A Natural History of Mind

The role of Evolutionary explanations in psychology

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Abstract

Evolutionary theory has been employed to explain psychological and social phenomena for over a hundred years. However, despite various claims that evolutionary theory should be considered the dominant theoretical framework for psychology, mainstream psychologists have resisted the widespread use of evolutionary explanations in their domain. This thesis aims to clarify the role of evolutionary explanations in psychology. In particular, I demonstrate that a clearer understanding of the role of evolutionary explanations in psychology is obtained by drawing on some recent literature in the philosophy of science. Evolutionary theory, I argue offers a coherent, unifying, explanatory framework for psychology, and evolutionary explanations should have a more prominent role in psychological science than they have had in the past. However, mainstream psychological theory will not be entirely replaced by theories drawn from the evolutionary research programme. The relationship between evolutionary explanations and other sorts of explanations in psychology is clarified, and some suggestions as to what evolutionary theory offers the future of psychology are forwarded.

Introduction

Humans are the product of evolution. Our opposable thumb, bipedal stance, and colour vision are all the result of millions of years of selection under ecological conditions where these characteristics proved favorable in fulfilling reproductive goals. The anatomical and physiological features of our bodies are clearly explicable in terms of evolutionary theory. Evolution by natural selection can explain why we possess a grasping hand with an opposable thumb, why we walk upright, and why we see the world in colour. In all motile organisms certain ways of behaving, but not others, are also likely to prove beneficial in terms of increasing reproductive success. Ultimately behaviour is regulated by brain processes. In organisms with complex nervous systems, brain processes can be conceptualised as the physiological instantiations of psychological mechanisms. Certain ways of perceiving and thinking about the world, of making decisions, and of integrating behaviour are likely to prove more reproductively successful than are others. It follows that evolutionary theory should be able to explain why humans think and behave in the way that they do. The logical conclusion is that evolutionary theory should be able to provide satisfactory explanations of human psychological and behavioral phenomena. Psychology should be considered as a branch of biology.

The sort of argument outlined above has been forwarded by numerous biologists, philosophers, anthropologists and psychologists since the publication of Darwin's *The Origin of the Species by Means of Natural Selection*. Darwin himself clearly viewed the emotional, behavioural, and psychological characteristics of humans as specific instances of evolution by natural selection at work. Evolutionary theory for Darwin, Huxley, Romanes, and others provided a means of explaining the nature and organization of the human mind just as it provided explanations for physiological, anatomical and behavioural phenomena in humans and other animals.

About a hundred years later, the development of sociobiology, to the consternation of many social scientists, offered a way of explaining the patterns of social behaviour in humans and other animals by reference to evolutionary theory. For E. O. Wilson, as with Darwin, psychology could be clearly conceptualised as a biological science. Just over a decade later, and in the wake of a voluminous raft of criticism directed at sociobiology, evolutionary psychologists were beginning to make similar claims. For evolutionary psychologists, however, the mind was seen as the level to which evolutionary explanations should be directed. The seemingly diverse behaviour manifest in humans was explained as the product of innate psychological mechanisms, explicable ultimately in terms of evolutionary forces. As with Darwin and Wilson, psychology was, and is, viewed by evolutionary psychologists as part of biological science. Evolutionary theory is, or should be, the unifying theoretical paradigm in the psychological sciences.

Mainstream psychologists, however, have been, and remain, unconvinced. Humans may be the product of evolution it is conceded, but that does not necessarily mean that the rich and diverse characteristics that humans possess are explicable in evolutionary terms. There is more to human life, it is suggested, than can be gleaned from an evolutionary point of view. The phenomena that psychologists study are necessarily imbued with meaning. Therefore, psychological phenomena are best understood in terms of specific developmental trajectories through richly structured cultural space. The role of evolutionary explanations, it is suggested, is extremely limited and psychology should not be conceived of as merely a branch of biology.

How are we to reconcile these two, seemingly contradictory perspectives on the role of evolutionary explanations in psychology? The fact of human evolution suggests some place for evolutionary explanations in psychology, but this by itself says little about just what that place might be. In this thesis I suggest that understanding the role of evolutionary explanations in psychology pivots on some central issues of more general concern in the philosophy of science. More specifically, the role of evolutionary explanations in psychology can be clarified by a consideration of the nature of theories and theory appraisal, the nature of explanation and how progress is achieved in science. If we are to understand what part evolutionary theory will play in a future psychology, we need to clarify just how evolutionary theory is structured, what its relationship is to other theories in psychology, and just what sort of explanations it offers for psychological science.

Philip Kitcher (1985) in his book *Vaulting Ambition* provided a detailed discussion of the nature and adequacy of sociobiology as an approach to human behaviour from a philosophy of science perspective. More recently, article length discussions of evolutionary explanations in psychology offered by philosophers of science include Stereleny (1992) Sober (1993a) and Kitcher (1990). Given the renewed interest in evolutionary approaches to psychology, I believe the time is ripe for a more detailed discussion of some of the crucial issues relevant to our understanding of the role of evolutionary explanations in psychology. Moreover, whereas past discussions have focused primarily on the adequacy of evolutionary explanations of psychological phenomena in methodological or epistemological terms, I believe is necessary to extend this analysis to a more in-depth treatment of issues of more global concern. To this end the aim of this thesis is to provide an evaluation of evolutionary explanations in psychology in terms of a more general conception of the nature of science. My primary goal is to provide a clarification of the role that evolutionary explanations play in psychology and the role that they should play in our attempts to further our understanding of psychological phenomena.

In the first chapter I provide a detailed discussion of the important conceptual issues relevant to evaluating the role of evolutionary explanations in psychology. My approach in this chapter, however, is more general in nature. I begin by providing a sketch of a realist approach to science. Although realism is rarely explicitly called upon in the rest of the thesis, it is an important perspective from which to develop a stable and coherent philosophy of science, and to locate psychology clearly as part of a more a more general attempt by science to understand the nature of the world.

Following this discussion of realism, I outline some contemporary approaches to understanding the nature of theories and theory appraisal. I draw heavily here on a model of theory appraisal recently forwarded by the cognitive scientist Paul Thagard (1992). Thagard's approach to theory appraisal provides a fruitful and illuminating way of understanding how we should characterise the relations between alternative theories and on what basis we should prefer one theory to another. In science, the unit of epistemic significance is rarely located at the level of single theories, but instead can be found in large scale or global theoretical constructions. At this point I offer a model of such large scale theoretical structures, which I call global research programmes. Global research programmes are shown to have important features that provide a means of understanding both the internal and external dynamics of science.

Clarifying the nature of theories and theory appraisal is important if we are to understand just what evolutionary theory offers for psychology. Questions regarding the nature of explanation are similarly relevant. Just when is something adequately explained, and what sort of explanations should we be seeking in science? I provide a discussion of these issues and present an approach to explanation that will prove useful in clarifying the kinds of explanatory resources that the evolutionary programme offers psychology. Finally in the first chapter, I present a discussion of the nature of conceptual change and progress in science. If we are to understand what evolutionary theory offers for the future of psychology it is important to clarify in what ways progress can be achieved and under what conditions we might say that a conceptual revolution in science has occurred.

In chapter two I discuss the nature of psychology. My aim is to suggest that psychology should be conceived as a science continuous with the biological and physical sciences. Psychology's subject matter indicates that it can only be considered as a semi-autonomous science, one with important connections to both biology and sociology. The subject matter of psychology also suggests that it will not be a science like physics with a primary focus on experimentation, prediction, and the construction of scientific laws, but should instead model itself on biology, with an emphasis on historical or narrative style explanations. The lack of cumulative progress in psychology, it is suggested, is partly a consequence of the acceptance of an inappropriate empiricist philosophy of science. The fragmented conceptual nature of psychology is also noted, and it is suggested that theoretical unification in psychology can be considered a virtue. However, unification is unlikely to be achieved through reduction and we must simultaneously pursue a practice of promoting theoretical pluralism. This discussion of psychology provides some conceptual background to an evaluation of the claims made by some in the evolutionary programme that evolutionary theory offers the appropriate unifying theoretical framework for psychology.

The history and structure of the evolutionary research programme is elaborated in chapter three. It is useful, I believe, to locate current attempts to employ evolutionary explanations in psychology in an historical perspective. Importantly, we need to understand why evolutionary theory has failed to gain widespread acceptance in psychology despite various attempts by philosophers and scientists over the last one-hundred years or so to explain psychological phenomena from an evolutionary perspective. The structure of the general evolutionary research programme is also discussed and disciplines such as sociobiology, behavioural ecology and evolutionary psychology are presented as clearly being part of the general evolutionary programme. There is considerable debate by evolutionary minded anthropologists and psychologists about how best to apply evolutionary theory to psychological, behavioural and social phenomena. I discuss some of the important issues in this context and promote an approach which encourages theoretical pluralism while drawing on the full resources of the evolutionary research programme in an attempt to establish the most explanatorily coherent way of explaining psychological phenomena from an evolutionary perspective.

In chapters four to nine I review the various criticisms which have been directed towards the evolutionary research programme in psychology. These criticisms are conceptualised in chapter four as being primarily concerned with the overall explanatory coherence of the programme. The various sorts of criticisms include questions regarding the moral acceptability of the evolutionary research programme (chapter five), the adequacy of comparative explanations in psychology (chapter six), the use of adaptation explanations (chapter seven) and role of learning and culture in explaining human phenomena (chapter eight).

In chapter nine I present a detailed discussion of the nature of jealousy and the various sorts of explanations that have been developed to explain jealousy-related phenomena. This chapter serves as a case study of the way I believe we should understand evolutionary explanations more generally in psychology. My focus, therefore, is on comparing evolutionary explanations of jealousy with alternative theoretical approaches in terms of some of the general criteria of theory appraisal outlined in chapter one. I also consider how evolutionary explanations of jealousy are related to other sorts of theoretical approaches. My conclusion is that evolutionary explanations have a prominent, but not exclusive role, to play in furthering our understanding of the nature of jealousy and the various phenomena that are associated with it.

The conclusions reached in chapter nine are adopted more generally in my overview of the role of evolutionary explanations in psychology in the final chapter. I present reasons to suggest that the evolutionary programme does not offer either a new or revolutionary paradigm for psychological science. However, I indicate that evolutionary theory needs to play a more prominent role in the construction of psychological theory and in developing explanations of psychological phenomena. I conclude by considering some of the ways that the evolutionary research programme might increase our understanding of psychological phenomena and provide a means of furthering progress in the psychological sciences. My conclusions may be more modest and circumspect than some in the evolutionary programme would like. Psychology will not be just a branch of biology. However, the conclusions are also more bold and expansive than many in mainstream psychology would allow. Psychology cannot ignore evolutionary theory. In developing a richer and more detailed understanding of humans and the world that we live in, it is important to make explicit just how, and to what extent, the evolutionary forces that have led to our existence provide satisfactory explanations of the phenomena which constitute the science of psychology.

Chapter One

Explanation, theory appraisal, and the growth of science: A realist perspective

In evaluating the role that evolutionary explanations have to play in psychology, it is important to provide a clear understanding of the nature of science itself. This is no easy task, for science has been characterised in a large variety of different, often mutually incompatible, ways. My aim in this chapter is to offer a coherent realist view of science. In particular, the focus of this chapter is directed towards an elucidation of the way science can be said to further our understanding of the nature of the world.

Firstly, I provide a discussion of the aims of science. Following Maxwell (1984), I suggest that science should be directed towards furnishing us with valuable knowledge of the world and our place in it. This, I suggest, is most fruitfully achieved given a realist philosophy of science. Realism, although the mainstream philosophical position, is currently under attack from a variety of sources. I suggest that realism can weather these criticisms and remains our best way of understanding the nature of science. A realist approach to science suggests that science advances our understanding of the world over time by providing increasingly better theoretical explanations of nature.

Following an overview of realism, I discuss the nature of theories and the way that they are organised into more large-scale conceptual structures. I offer at this point a model of global research programmes in science, which provides a means of understanding the way that scientific theories are organised, and the relations that obtain between academic disciplines and between science and society. An overview of the nature of theory appraisal in science is then presented. I suggest here that a model of theory comparison developed by Thagard (1992) provides a fruitful way of understanding the relative value of theories and the various kinds of relationships that obtain between them.

The idea that science provides us with an understanding of the world via the development of theory is tied closely to the notion of explanation. I discuss the different ways that scientific explanation can be conceived and forward a dual perspective on explanation championed by Kitcher (1985, 1989) and Salmon (1989). This approach to explanation provides us with a way of realising how science furnishes us with a means of increasing our understanding of the world. Finally, I discuss the nature of progress and conceptual change in science. The discussion here pivots on the way that science can be said to provide us with progressively richer and more detailed accounts of the way the world is.

The aims of science

One plausible way of characterising science and demarcating it from other forms of intellectual inquiry is with respect to its aims. However, just as science itself can be viewed as a highly heterogeneous collection of activities, so too can its aims be variously construed. Generally speaking empiricists conceive the goal of science to be the prediction and control of observable data. Successful theories are to be judged solely with respect to their empirical adequacy (van Fraassen, 1977). On this instrumentalist construal of theories the aim of science can be viewed as the production of ever more powerful descriptions and orderings of empirical results.

For Laudan (1977) the aim of science is the solving of empirical and conceptual problems. Progress in science can be judged to the extent that empirical problems are solved and anomalies and conceptual problems are minimised. Kuhn (1962) also sees science ('normal science' at least) as essentially a 'puzzle-solving' activity. Science on this view may progress in some sense, but it doesn't progress *towards* anything, such as truth.

In contrast to this perspective, realists, in general, characterise the goal of science as the discovery of explanatory truths about the world (Newton-Smith, 1981; Boyd, 1984). Realists claim that science seeks to provide an increase in *understanding* about the nature of the world. The instrumentalist's goal of increasing the empirical content of scientific theories is subordinate to this broader aim. Given the inherent fallibility of theories and their modification over the history of science, it is unlikely that at any point in time we can claim that we have a true theory. Progress in science, therefore, is often conceived of as the increase in approximations to the truth, or the increase in the 'verisimilitude' of theories (Popper, 1963).[1]

Both the instrumentalist and orthodox realist perspectives on the aim of science capture something of the nature of actual scientific practice and provide a rationale for scientific inquiry. However, I think they both do some injustice to the nature of science as an essentially human activity, and to the way science should, indeed needs to, develop in the 21st century.

An evolutionary approach to science (e.g., Hooker, 1987; Dunbar, 1995) suggests that science is essentially a species strategy for coming to understand the nature of the world and our place in it. Like all organisms, humans subsist on information, we are 'informavores'. The desire for ever better representations of ourselves and the world we live in is a fundamental feature of our evolved biology. When this 'epistemic hunger' (common to all organisms not entirely pre-wired) is coupled with human reflexivity and language then we have the essential preconditions for the distinctively cumulative and progressive nature of science. Information about ourselves and the world we inhabit, however, is from an evolutionary point of view, never neutral: we are not seekers of truth *per se*, but of *valuable* truth. Ultimately what is of value to us is open to question and revision, but the best place to begin an understanding of value is with evolution: the

[1] Such a notion is not without its problems (see Newton-Smith, 1981; Laudan, 1984), however, I defend something like a 'horizon' concept of truth in my discussion of realism below. Although truth is never ultimately obtained, theories can be said to have this aim in their 'sights'.

ultimate source for an explanation of what is good or bad for organisms of our type in the kind of world we inhabit.[2]

A construal of science which does justice to the importance of valuable truth (although without the overt evolutionary overtones) is the fundamentalist approach articulated by Nicholas Maxwell (1980, 1984). Maxwell argues that the goals of science as typically conceived: prediction, control, explanatory truth and so on, although important in their own right, need to be subordinate to still more fundamental aims.

> Ideally intellectual inquiry ought to help us to tackle rationally those problems of living which we encounter in seeking to discover and achieve that which is of value in life. Intellectual inquiry ought, in other words, to devote reason to the enhancement of wisdom. (Maxwell, 1980 p19).

Maxwell's fundamentalism suggests that all of intellectual inquiry should ultimately be directed to answering four basic questions:
(I) What kind of world is this?
(II) How do we fit into the world and how did we come to be?
(III) What is of most value in life and how is it to be achieved?
(IV) How can we develop a better human world?[3]
Just how best to answer these questions and how to achieve the ultimate aim of developing what is of most value in existence is of course open to discussion, revision and change. However, we will never answer our fundamental questions of existence unless all of scientific inquiry is aimed ultimately at these four basic questions outlined above.

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[2] I am not arguing here that this is the only source for an understanding of value, for there will always be conflicts between what is reproductively good for us and what we as individuals value. Moreover, values may be relatively idiosyncratic to local features of culture at a level too fine-grained to make evolutionary explanations particularly useful.
[3] Maxwell is somewhat anthropocentric here. This fourth fundamental question should I believe be directed at developing a better world for humans and all other life forms on the planet.

Maxwell contrasts this fundamentalist approach with the specialism typically encountered in science. Science in its twentieth century guise consists of a large number of highly specialised disciplines and sub-disciplines all concerned with answering highly specific and technical questions within their domain. Specialised inquiry is not a bad thing in itself, indeed it is necessary to answer question one above; however, this kind of problem solving should not be divorced from a consideration of more fundamental assumptions and problems. It is important to note that Maxwell is not endorsing some kind of simple pragmatism here. Intellectual inquiry is also to be pursued for its own sake. 'Pure' intellectual inquiry is a part of what is of value in living. Born in to the world in a state of ignorance,[4] our fundamental problems centre around our developing an understanding of what we are and how we relate to the physical, cognitive and social worlds around us. Ultimately, however, the way specialised inquiry is conceived in contemporary academic institutions needs to be radically restructured along non-specialised lines.

Maxwell suggests that disciplines can no longer remain isolated from one another in their pursuit of highly specialised knowledge of the world. Moreover, they must openly acknowledge their multiple dynamic relations to other areas of inquiry, to institutions, to society and more globally, to the planet itself. There needs to be reciprocal lines of discourse between experts and non-experts; between institutions and scientists; and more generally between society and science. I shall suggest, below, that one way of achieving these desiderata is through the development of global research programmes which, among other things, specify ways in which sub-disciplines are related to one another and which provide means of relating the accounts of science to specifically social and moral issues. The aim of achieving valuable knowledge, moreover, is best realised given a realist construal of science.

[4] Well, comparative ignorance at least. We do, from birth, have some understanding of some basic features of the world we inhabit (e.g., see Spelke, 1991; Shepard, 1984). And of course just how we come to *learn* about the world is to some (as yet unspecifiable degree) *framed*. (see the contributions to Carey & Gelman, 1991).

A Realist perspective

Both lay-people and working scientists tend to be realists (in some respects at least). Realism is also the majority position in the philosophy of science. However, it is currently under attack from both relativists and empiricists, although for somewhat different reasons. In this section I outline, and defend, a realist approach to science from various anti-realist moves. In particular, I highlight the role that super-empirical values play in theory appraisal and in the growth of science. I argue that our best understanding of theory evaluation and progress in science is most coherently understood from a realist perspective.

Realism, as the title of a recent book by Rom Harrè (1986) suggests, comes in a variety of forms.[5]Ontological realists believe that the physical world has an existence independently of our perception and thoughts about it. This is perhaps the central idea of realism, and is one embraced by lay-folk and most philosophers and scientists alike. However, the idea that reality exists independently of knowing has been denied by some. Most notably by Berkeley, but also by some post-modernist philosophers.[6]

Ontological realism with respect to theoretical entities is the idea that the non-observable entities described by scientific theories have an existence in the same sense that observable objects do. Most working scientists, as Hacking (1984) notes, clearly are realists in this sense. Experimental physicists design equipment and experiments which can manipulate unobservable entities in such a way as to produce new phenomena. The manipulation of entities in this manner assumes that they actually exist. Some sociologists of science, however, (e.g., Latour & Woolgar, 1986) deny the reality of entities in these cases. Rather than scientists *discovering* entities such as electrons in the

[5] My outline of the central ideas of realism is drawn variously from the overviews provided by Hooker (1987); Boyd (1984); Leplin (1984); and Greenwood (1987).
[6] For example, Woolgar (1988) and Gergen (1985), who deny that reality can, in any meaningful sense, be said to exist prior to and independent from, a suitably constructed knower.

world, such entities are conceived of as being *constructed* through a social process of negotiation and persuasion.

Although there are some questions with regard to the status of ontological realism, it is with respect to epistemological and semantic realism that most objections have been directed. It is here also that many realists (who share similar ontological commitments) part company. Epistemological realism is the thesis that we can have knowledge, albeit highly conjectural and fallible knowledge, about the nature of theoretical entities. Our best theories therefore are also our best guides to reality. Moreover, because theories refer to non-observable entities, it is the global excellence of a theory, not merely its empirical adequacy which provides the basis for its acceptance or rejection. Most empiricists (notably in recent years, van Fraassen, 1977, 1984) reject this approach to theoretical entities. For them, empirical adequacy is to be our primary, if not exclusive, guide to the acceptance or rejection of theories. We can have no knowledge of unobservables.

The third major realist thesis, semantic realism, claims that our theoretical terms embodied in language are genuinely referential in nature. Truth, therefore, consists in an appropriate correspondence relation between language and the world. Theories are true, or approximately true, by virtue of the way the world is.[7]

This point leads to another central claim of realists: the progressiveness of science in terms of the increasing truthfulness of theories. Typically speaking, it is claimed, scientific change is progressive in nature. Successive theories in the history of science are successful to the extent that they provide, generally speaking, a better account of the nature of the world, in both its observable and non-observable features. Indeed, as many have argued (e.g., Hooker, 1987; McMullin, 1984), the success of science in purely pragmatic terms, is only explicable given a realist construal of science. That is, scientific

[7] It is here that one realist at least (Harrè, 1986) parts company. For Harrè, questions of truth are not questions about the relations between language and the world, but are questions abut the moral nature of scientific communities.

theories must be at least approximately true. Progress in science then consists in the convergence of scientific theories on the true nature of the world.

In what follows below I defend a strong form of realism, which endorses the ontological, epistemological and semantic aspects of realism outlined above, from various anti-realist criticisms.

Anti-Realism

Challenges to realism come in a variety of guises, manifested as rejections of the ontological, epistemological and semantic aspects of realism outlined above. These challenges can be either local or global in nature. The ontological challenge to realism, in its global form, represents a rejection of the idea that the world exists independently of a suitably constructed knower. Local anti-realists, in contrast, question the existence of *parts* of the world assumed to exist in realist doctrines; those parts which are unobservable to unaided humans senses. The epistemic challenge to realism questions our ability to know the truth or falsity of theories, either globally, or as they refer to specific hidden entities. Doubts about whether terms in theories can be said to refer to theoretical entities represents the semantic challenge to realism.

These doubts about the adequacy of scientific realism are probably best understood in terms of the critiques developed by alternative scientific methodologies. For convenience, I group these challenges roughly into two alternatives: empiricism and relativism, while appreciating that there is a diversity of positions represented by these two approaches. However, despite differences in the details of different versions of empiricism and relativism, the anti-realist critiques of these approaches can be fairly clearly articulated.

The challenge from empiricism

The anti-realist in the empiricist tradition provides a local challenge to the epistemological and semantic theses of most realist doctrines. Empiricists question the realist claim that we can have knowledge of unobservable entities, and that our theories can be licitly said to *refer* to those entities. Typically speaking, empiricists do not question the *existence* of non-observable entities, however, they argue that *knowledge* of these entities is not possible.

In recent years, this perspective has been most forcibly championed by van Fraassen (1977, 1980, 1984), with an approach he labels *constructive empiricism.* For van Fraassen, the acceptance or rejection of a scientific theory should be made solely on the basis of its empirical adequacy. Reference to non-observable entities and to virtues above and beyond empirical adequacy have no role to play in theory appraisal and the growth of science. Theories which include non-observable entities in their ontology will always be radically underdetermined by their empirical adequacy, as many theories which may be logically incompatible will be empirically equivalent. Van Fraassen claims that no scientific evidence can bear on the adequacy of theoretical claims about non-observable entities, therefore, the ontology of our theories should be purged from all such claims.

There remains of course the question of just what should count as being observable. For van Fraassen the distinction should be drawn relative to us. Claims about what is observable should be made in reference to the normal unaided perceptual capacities of humans. Van Fraassen justifies this anthropocentric distinction by noting that it is *our* attitudes towards theories which determine their acceptance or rejection. Scientific activity is best conceived of as construction rather than discovery; the construction of theories which are adequate to the data.

The challenge from relativism

Anti-realism in the relativist tradition, despite the variety of positions adopted, pivots on two principle arguments: the underdetermination of theories by evidence and the theory-ladenness of observation (Knorr-Cetina & Mulkay, 1983). The Duhem-Quine thesis states that because theories are never tested in isolation, but are always conjoined with auxiliary hypotheses, and based on methodological assumptions, evidential equivalence between theories can always be maintained through modifications or replacement to some aspect of the theory matrix. Theory choice, therefore, can not be based on empirical adequacy alone; other factors must be involved.

For sociologists of science, these other factors are often, but not always, social factors: Factors that lie outside the rational choice of theories as best representing the true nature of the world (Knorr-Cetina & Mulkay, 1983; Mulkay, 1979). Such social factors may include the motivation and ambition of individual scientists, the social allegiances of scientists to class systems, and the political climate in which such decisions are made. The acceptance of Lamarckian theory in soviet biology is the classic example of the overriding force of political factors in theory acceptance.[8] However on a smaller scale, in the everyday practice of science, a variety of external factors are said to play a role in the epistemic attitudes that scientists adopt towards their theories.

From this perspective, the realist's claim that theories in some sense map an external reality, and that successive theories in the history of science represent increasingly better representations of the world, is called into question. If factors other than the epistemic virtues of theories play a major role in theory acceptance and rejection, then the so-called progress of science better reflects changing socio-political circumstances than it does the increasing truthfulness of theories.

[8] Bloor (1981) argues that the invocation of social factors to explain belief acceptance, however, is not just restricted to cases of bad science.

The possibility of a central role for social factors in the process of theory appraisal also follows, so relativists claim, from the theory-ladenness of observation. Observations, the argument goes, are never theory neutral; they always presuppose some prior ontological commitments. Although this point was made explicit by Popper (1959) in his critique of logical positivism, it is with Kuhn (1962)[9] and Feyerabend (1975) that the theory dependence of observation has been deployed as an argument against the rational and progressive nature of scientific inquiry.

If scientific theories provide an intellectual structure which serves to organise and interpret observations, then there can be no theory-independent observations on which choices can be made between rival theories. Theories are said to be *incommensurable*, and scientists, in Kuhn's (1962) vivid metaphor, are said to exist in different worlds. Facts are not out there in the world as such, but are constructed through a social process mediated inevitably by theory. From this perspective, theory is viewed as permeating science's observational base.

Relativists see the theory dependence of observation as providing a fundamental challenge to the epistemological core of realist doctrine. Theories cannot be meaningfully thought of as representations of an independently knowable reality, if the reality that scientists measure is to large extent an artefact of prior theoretical commitments (Woolgar, 1988). This challenge to the epistemological base of realism has been extended to embrace realism's ontological commitments. Woolgar (1988 p. 60) argues that the natural world is constructed through, rather than revealed by, its discovery. ". . . the fact of an object is the temporarily stable upshot of a complex social process. Moreover, this process continues long after an initial discovery claim, 'the object' has and will continue to change."

[9] Kuhn's position regarding the rationality of theory change has changed somewhat since the first edition of scientific revolutions. In later works Kuhn (1977) defends the rationality of theory appraisal in terms of some enduring epistemic aims.

The character of an object, Woolgar argues, changes with different stages of research. The 'discovery' of quasars, for example, was not a case of *revealing* some hitherto unknown aspect about the world, but of *constructing* it through a social process of negotiation. At various stages of the research the 'object' was labelled as: an unusual trace; possible interference; unusual interference; communications from another civilisation; and some new kind of pulsating radio source. In this, and other situations, so it is claimed, the social network constitutes the object which does not exist antecedently and independently of the construction process.

This brief review does not do justice to the various kinds of positions adopted by relativist critiques of realism. However, although the strength and significance of the theory dependence of observation and the underdetermination of theories varies, as a challenge to realism, all relativists question the notion explicit in realist doctrine, that science in some sense can be said to be providing us with increasingly better (more truthful) representations of reality.

The challenge from the history of science

Realists are typically sanguine about the history of science. Successive theories over times are seen as providing us with increasingly better representations of reality. Science is converging on the truth. This optimistic induction on the history of science has been challenged by Larry Laudan (1984). Laudan argues that many successful theories in the history of science have been non-referring. The pragmatic success of a theory therefore, can not be indicative of its truth status. Moreover, genuinely referential theories are not always successful at the empirical level. Wegener's theory of continental drift is a notable example of just such a theory. Although it is genuinely referential in nature, it remained unsuccessful for over thirty years. A truly referential theory may also, Laudan argues, make false claims about the nature of the world. In sum then, the referential status of theoretical terms do not provide us with any kind of guide as to the acceptance and rejection of theories as realists claim.

Laudan also questions the notion of approximate truth. What exactly does it mean for a theory to be approximately true? If a clear notion of truth cannot be explicated, then it is difficult to see how the truthfulness of a theory can explain its success. The notion of approximate truth is also problematic when considering the progress of science. Laudan claims that successive theories in science do not typically retain the key mechanisms and theoretical entities of prior theories. If this is the case, the idea of cumulative progress towards the truth is difficult to sustain. In what sense can we say that our present theories are truer than those that have gone before? Realism, it is argued, simply cannot provide an adequate explanation of why science works.

Realism defended

The status of non-observables

The local anti-realism espoused by van Fraassen, suggests that we should limit the domain in which we can reliably generate true beliefs to what is observable. But on just what basis can this selective scepticism be maintained? As Churchland (1989) points out, given the theory dependence of observation, the epistemological status of observable entities is rendered just as dubious as that of non-observables. Why should our cognitive limitations extend only to those entities which are not immediately perceptually accessible to us? There also seems no reason why the data generated via perceptual prostheses (such as telescopes and microscopes) should be significantly less reliable than those generated by our unaided senses. Both the eye and the telescope provide us with information about the world, which in some sense is dependent on, and mediated by, theory of some kind.

The absolute distinction between observables and non-observables is also difficult to maintain. As Churchland (1989) argues, van Fraassen cannot make a principled distinction between things that are possibly observable by humans (with unaided senses), and things that can not be in *principle* be observed by humans. Our epistemic attitude to dinosaurs, electrons, continental drift and the oort cloud should, therefore, be the same, despite their different observational status. It follows that even the empirical adequacy of a theory is something that is radically underdetermined by the evidence. A theory cannot say anything about *all* observable entities in space and in time.

More broadly speaking, the realist's confidence about unobservables is based on the considerable success of theories in the history of science that refer to hidden entities. Reference to genes, electrons, thoughts, dinosaurs and the like, provide the best explanation of phenomena that scientists seek to explain. Moreover, our ability to manipulate those entities, such as genes and electrons, so as to produce new phenomena, provides us with further reason to accept their equivalent epistemic standing with more manifestly observable entities (Hacking, 1984; Giere, 1988).

The role of super-empirical values

Both empiricists and relativists use the Duhem-Quine thesis of underdetermination to support their anti-realist claims. Empiricists argue that reference to non-observables creates an unacceptable proliferation of ontologically diverse, but empirically equivalent theories. Relativists, on the other hand, argue that underdetermination of theories by their evidential base, strongly implicates the role of external (i.e. social and political) factors in theory appraisal.

The realist's response to these claims is to suggest that the fact of underdetermination neither necessitates a rejection of hidden entities, or an invocation of social factors in theory choice. Instead, the underdetermination of theories by evidence, suggests the vital role that super-empirical values play in theory choice (McMullin, 1993; Churchland, 1989). Such super-empirical values include simplicity, explanatory breadth, consistency, and fertility. It is the global excellence of a theory, not merely its empirical adequacy, which provides a measure of that theory's success. I shall have more to say on the role of super-empirical values in my discussion of theory appraisal below, but analyses of the history of science (e.g., Thagard, 1992; Giere, 1988) strongly implicate values beyond those of empirical adequacy as being crucial in the acceptance and rejection of theories. These same studies, although not denying the role of social factors in science, suggest that it is the epistemic values internal to science which do most, if not all, of the explanatory work in accounting for theory change in science.

For both Laudan (1977, 1981) and Lakatos (1970), sociological factors can only be employed to explain the content of *bad* science; there is no need to invoke social factors in an explanation of adequate science. The acceptance of a Lamarckian theory of inheritance by soviet biologists then, requires an explanation in terms of socio-political climate of the time. Most theory choice in science, however, can be adequately explained by those values which are said to be internal to scientific inquiry.

Even Kuhn (1977), who is often cited as invoking social factors in theory choice, argues that it is typically those internal virtues of a theory that provide the basis for theory acceptance and rejection. However, just how those criteria are employed, may vary from scientist to scientist, depending on the social context and other external factors. Recourse to individual biography to explain theory choice, argues Kuhn, does not mean these choices are *subjective*, at least not in the sense that they cannot be discussed as something over and above matters of mere *taste*.

Ultimately, the role that social or external factors play in theory choice is an open question; one that needs to be analysed on a case by case basis. However, the history of science does provide some support for the idea that it is typically those values internal to science, such as explanatory breadth and simplicity which play the major determining role in scientist's epistemic attitude towards theories. That is not to deny that some external factors may play an important role in theory appraisal. However, there is no suggestion that factors considered external to science (ambition, political allegiance and so on) typically *overwhelm* values internal to science, when scientists make decisions about theory acceptance or rejection.

A closer look at the theory dependence of observation

The theory-dependence of observation has been used widely by sociologists of science as a critique of the assumption that there is some neutral observation base on which theory choice can be made. Following Kuhn (1962), it is suggested that the theory or paradigm adopted determines the nature and content of the observations made. It follows that comparisons between theories in terms of the claims they make about the world cannot be fruitfully made.

> Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for those depend in part upon a particular paradigm, and that paradigm is at issue.
> (Kuhn, 1970, p. 94).

If this is truly the case, then of course it is difficult to uphold the realist's claim that science progresses through theories which provide increasingly better representations of the world. After all, not only are theories said to be incompatible, but they determine the kind of world in which observations are to be made.

One possible way of defending realism from such a challenge is to question the claim that observations really are theory laden. One defence of the autonomy of observation from theory is provided by Jerry Fodor (1984). Fodor argues that our early perceptual processing capacities are insulated from those higher order systems, so that our concepts do not penetrate our perceptual systems. Fodor points out that many illusions maintain their illusory status, from a perceptual perspective, even if we *know* that our sense are being fooled.

However, as Churchland (1989) has persuasively argued, such a defence will simply not work. For a start, it is highly questionable that our perceptual systems really are independent of higher order processes. Many illusions *do* change their character given the requisite knowledge. Moreover, the role of conceptual knowledge in the interpretation of observations through telescopes, microscopes, and of x-rays, is well documented (e.g., Chalmers, 1982).

Churchland (1989) points out, furthermore, that even if there was considerable rigidity at the perceptual level, there still might be considerable *conceptual* plasticity in terms of how those sense data are ultimately processed. The number of possible mappings from percepts to concepts leaves plenty of elbowroom for multiple interpretations of reality. It seems that to some extent perception is theory laden. Indeed, given the idiosyncrasy of our evolutionary history, this thesis is almost trivially true. What is observable to us, is dependent on the complex collection of theories which explains the functional capacities of our perceptual-cognitive system.

There is no need for the realist to reject the idea that observations are to some extent theory-laden. What is truly problematic for a realist ontology, is the idea that observations are *determined* by a process which is idiosyncratic to the social context (including the theoretical framework) of the scientists concerned. This is what lies behind the claim of some sociologists of science (e.g. Latour and Woolgar, 1986) that facts are, to all extents and purposes, socially constructed.

Talk of facts being socially constructed, however, fails to make an adequate distinction between phenomena and data. Data is variable and represents the outcome of various experimental interactions with reality. Phenomena, on the other hand, are stable, repeatable, aspects of the world (Bogen & Woodward, 1988). That data changes over time, as in the discovery of quasars, is not surprising. However, it is not the character of the object that changes *per se*. What we see in the process of scientific discovery is different interpretations of multiple patterns of data. The upshot of this process is the discovery of a new phenomenon: quasars. Sociologists of science such as Woolgar (1988) are therefore making the illicit move from the theory dependence of observations to the theory dependence of nature (Kitcher, 1993).

That knowledge of the world is socially constructed, is to some respects trivially true. Scientists are social beings operating in an institutional environment embedded in a culture. Theories are, *prima facie*, social constructs; they are constructed partly through the enactment of social processes. That such knowledge is arbitrary and idiosyncratic to a particular social milieu, that is, that the world can be so variously interpreted as to make theories only analysable from a socio-cultural perspective, is however, to be rejected. The importance of taking into account the social dimensions of science does not necessarily entail a rejection of realism and its acceptance of an independent reality, which is to some extent knowable. Between the two extremes of a world fixed by its empirical content, and one multiply constructed by communities of scientists, there is ample room for a realist portrayal of science which does justice to both.

As Kitcher (1993) notes, there is a vast unexplored middle ground between the extremes of an approach which denies the role of social factors, and one which views nature as virtually impotent in determining the nature of theories. The strong version of the role of social factors in science is surely false. Scientists with the same social background differ in their beliefs due to different encounters with the world. Moreover, scientists do find out things quite contrary to their expectations. What we believe does not necessarily *dictate* what we perceive (Kitcher, 1993).

Furthermore, the history of science suggests that even scientists with quite different theoretical commitments can agree about what is to count as significant observations. For example, despite significant differences in the ontology of the phlogiston theory compared to its oxygen counterpart, both Lavoisier and Priestly could communicate easily enough about the results of their experiments, and agreed on what would count as significant results (Thagard, 1992).

What needs to be examined in more detail is the nature and extent to which theories permeate observations both in science and in everyday life. This is ultimately an empirical question, and one of interest to psychologists, sociologists, philosophers, and historians alike. However this preliminary analysis suggests that the world is robust enough to provide the primary means whereby scientists can make rational choices about theories.

Rescuing truth: an optimistic reappraisal of the history of science

Laudan (1984) provides us with a pessimistic induction on the history of science. There may be progress through time, but that progress cannot be conceived of as progress towards truth. Laudan's analysis rests on the claim that the referential status of theoretical terms plays little part in the acceptance or rejection of theories, and fails to be retained in successive theories. More broadly speaking, the history of science suggests that all theories in the past have been false, and there is nothing to suggest that the epistemic status of our current theories is any different.

Although the possibility of achieving absolute truth is to rejected, realists typically still hold onto the notion of a convergence on truth. Our current theories may be false, but they are less false, or more truthful, than those they have replaced. As Kitcher (1993) notes, it is not enough to demonstrate the falsity of all past theories, the anti-realist also needs to demonstrate that the history of theoretical entities has been *erratic* in nature.

> The history of science does not reveal to us that we are fallible in some undifferentiated way. Some kinds of claims endure, other kinds are likely to be

discarded as inaccurate. Furthermore this is exactly what we would have expected given our conception of the relationship between human cognitive systems and nature.

(Kitcher, 1993 p. 138).

One of the goals of a realist approach to science, is to uncover just those aspects about reality which we can be more or less sure about. In many ways our abilities to generate true theories may be domain-specific. Blanket claims of pessimism (or indeed optimism) regarding the truth status of theories are simply not warranted.

The history of science, contra Laudan, does provide some evidence for the idea that successful theories or parts of theories are those in which the central terms truly refer (McMullin, 1984). When we consider theories at a more fine-grained level, we see that some parts are more successful than others, and that these parts of theories are often the ones that are genuinely referential in nature. Moreover, these successful aspects of theories are maintained across time. For example, early twentieth century geology was successful in many areas in which the acceptance of a false stabilist position had no great impact. Less success was met with when geological theory had to work within false stabilist constraints (Kitcher, 1993).

Certainly science does not, and probably never will, give us the whole true picture. However, a more modest realism provides us with some encouragement for the idea that we are obtaining better representations of the world. As long as we accept the idea that our ability to choose between theories, or parts of theories, is predicated on methods that are to some extent reliable (in terms of producing verisimiliar representations of the world), then realists claims for convergence can be maintained.

Theory appraisal and the role of global research programmes in science

All our knowledge of the world is obtained via theory. Theories are the vehicle whereby we arrive at an understanding of the nature of reality. A realist approach to science views theories as explanatory in nature. In contrast to the descriptive role of theories in empiricist philosophies of science, realists argue that science advances through the articulation and development of deep-structured or postulational theories; that is, theories which refer to latent entities and provide us with *explanations* of phenomena.

Realists typically adhere to a correspondence approach to truth. Our theories, in some respects, and to some degrees, *correspond* to aspects of the real world. This correspondence is best considered as a mapping rather than a mirroring relation (Hooker, 1987). From an evolutionary perspective, the ability to develop maps or representations of the world is a natural capacity of information processing systems like humans and other animals. To further the goals of survival and reproduction many organisms have evolved abilities which transcend mere hard-wired responses to local contingencies in the environment.[10] The ability to plan sequences of action, in even the most rudimentary sense, requires some knowledge of the world. This knowledge is acquired via the dynamic process of gene-environment interaction during the ontogeny of the organism's nervous system. The notion of correspondence then, is not between sentences and the world - theories are not fundamentally linguistic entities - but between internal maps or models, and an external reality.

The idea of theories as models, or populations of models, has received considerable support from a variety of realists (e.g., Churchland, 1989; Giere, 1988). Given that it is unlikely that sentences form the basis for *most* of our, and *all* of other organisms' cognitive activities, it seems more plausible to suggest that theories are in some sense

[10] Whether or not animals possess representations of the world is currently a matter of some debate. However, the burgeoning literature in animal cognition (e.g., Ristau, 1992; Griffin, 1984) provides considerable support for the continuity or correspondence of many animal species with humans in this respect.

models of the world that enable organisms to interact with external reality in order to further their evolutionary goals.

Models as candidates for truth, then, can be accepted or rejected on the basis of the extent to which they are similar to external reality. Similarity here can be specified in terms of various respects and degrees (Giere, 1988). Models, or better populations of models, can be similar to the world in some respects but not in others. Moreover, although our theories are likely to be false, this falseness is not an all or nothing affair, but instead comes in degrees. Often our first initial probings of an aspect of reality are widely accepted to be way off the mark, while still providing a useful first approximation as to how that aspect of the world is structured. The evolution of climate models is instructive in this respect. Initial models provided crude global averaged estimations of changes in temperature based on a few, reasonably well-known parameters, such as concentration of atmospheric gases, cloud reflectivity and solar energy. Later models, the so-called general circulation models, have been able to provide detailed local predictions by including an increasing variety of relevant parameters: including life-climate feedback systems, ocean currents and more detailed analyses of cloud effects. Although the initial models were ultimately false representations of the world, they provided some measure of similarity: temperature in the real world *is* affected by levels of atmospheric gases and solar radiation. Later models, while still false, furnish us with greater degrees of similarity between our representations and the world by taking into account a wider variety of systems which affect climate in the real world.

The notion of theories as models also provides a further means of rebutting some of the anti-realist arguments outlined earlier. The advancement of science can be seen in terms of increasingly better models of the world. Where better refers to some increasing similarity between the model and nature. So it follows that even rejected theories are not entirely false, but simply lack strong similarity, or the right kind of similarity in comparison to proffered alternatives.

> One way science advances is by discovering new aspects of the world, that is, new respects in which our models might resemble the world. Science also advances by discovering some respects in which similarities between model and the world are *not* as commonly thought.
>
> (Giere, 1988, p.107.)

In at least one important respect the history of science is analogous to the ontogeny of human cognition. Through the developmental trajectory of both runs increasingly richer and more complex sets of models which provide better maps with reality. These maps provide, for both scientists and developing children, increased understanding of the world of which they are a part.

Scientific research programmes

On the realist account of science I am presenting here, theories can be fruitfully considered as modelling aspects of the external world. However, in science theories rarely, if ever, occur in isolation. Instead, they are typically conjoined with other, auxiliary theories, methodological assumptions, scientific laws, and so forth. It follows that in evaluating theories, the unit of epistemic significance is rarely the individual theory itself, but rather is the whole network of assumptions in which it is embedded.

These larger networks of assumptions has been variously termed 'paradigms' (Kuhn, 1970), 'scientific research programmes' (Lakatos, 1970), 'research traditions' (Laudan, 1977), and 'global theories' (Hooker, 1975). Although in many respects similar, these different approaches also provide some interesting points of departure that are worth elaborating on.

Kuhn's (1970) notion of paradigm has been widely influential among philosophers and scientists. However, as commentators on Kuhn have noted, his use of paradigm in the structure of scientific revolutions is highly variable. Masterman (1970), for example, enumerated twenty-one different usage's of paradigm employed by Kuhn. Kuhn (1977) himself is aware of this variable usage and distinguishes between two main meanings for paradigms. The first construal of paradigm is global in nature and refers to the shared commitments of a particular scientific community. These commitments include shared goals, epistemic values and so forth and are typically underpinned by a common educational background. Paradigm, in the second main sense intended by Kuhn, refers to more specific kinds of commitments shared by scientists. These sorts of commitments include various cognitive components such as models, symbolic generalisations and exemplary instances of scientific work. Kuhn also uses the term 'disciplinary matrix' to refer to this shared collection of cognitive values. It is this second usage of paradigm which captures most of what we mean by the Newtonian paradigm, or the Darwinian paradigm in science.

In his earlier work, Kuhn also makes a distinction between paradigms and schools. The pre-paradigm phase of science is characterised by collections of poorly articulated and generally limited theories which are pursued by different schools. In later work, Kuhn (1970, 1977) abandoned any hard distinction between schools and paradigms, appreciating that schools are most fruitfully thought of as poorly articulated and limited paradigms.

Paradigms for Kuhn provide, very generally speaking, a way of organising and observing the world. What is considered as observable and how observations are interpreted, is going to be partly a consequence of the paradigm adopted by the scientist. Paradigms then, loosely characterised, are overarching frameworks from within which scientists can carry out the practice, of what Kuhn terms 'normal science': the pursuance and solving of empirical and conceptual puzzles. Paradigms are overthrown or replaced during times of scientific revolution, when a competing paradigm becomes accepted by a significant portion of the scientific community. It is here that Kuhn's account of science has been charged as irrational. Certainly in his early work Kuhn has characterised this process of scientific change as one of conversion or of gestalt switching. However, as he has made clear in later works (e.g., Kuhn, 1977), the acceptance of a new paradigm is normally due to its greater worth in terms of satisfying a set of fairly stable epistemic values, such as simplicity, explanatory breadth and fertility. It follows that the influence of a paradigm is not so all embracing that followers of competing paradigms cannot communicate intelligently with one another.[11]It is also clear that observations can, in some sense, provide a theory neutral basis from which comparisons can be made.

Lakatos's (1970) notion of scientific research programmes captures many of the aspects of Kuhn's paradigms. Like Kuhn, Lakatos emphasises the importance of appraising larger-scale theory clusters rather than isolated theories. As such, Lakatos's methodology of scientific research programmes is intended as an extension of Popper's falsificationism. It is research programmes however, not individual theories, which are candidates for falsification.

A Lakatosian research programme has two methodological prescriptions. The negative heuristic outlines what paths a scientist can avoid in their investigations. In particular, the hard core of a research programme is considered as immune from falsification, and is not directly under test. The hard core of a research programme contains those central assertions (often embodied in laws) which provide the basis for understanding the rich array of theory and phenomena which embody the research programme and its domain of inquiry. The hard core of the Darwinian research programme, for example, is the idea of natural selection coupled with the laws of descent. Empirical results which may seem to falsify the hard-core assumptions should be accommodated by the research programme by modifying the 'protective belt' - the collection of auxiliary assumptions, methodological rules and so forth, ancillary to the central claims of the programme. These changes to the auxiliary assumptions of a research programme should be content increasing and not *ad*

[11] Kuhn draws the analogy here between speakers of different languages. Although it is possible to translate from one language to another, this translation inevitably involves some leakage of meaning. The adequacy of a translation is typically determined by the degree to which the individual has immersed themselves in the culture in which the language is a part. To adequately understand the nature of some alternative paradigm then, one must step outside one's own paradigm and become part of the alternative paradigm's culture.

hoc in nature. The anomalous phenomenon of altruism for example in biology, rather than spelling doom for the Darwinian research programme, was accommodated by the development of the theories of kin and reciprocal altruism. These theories greatly expanded the range of biological phenomena which could be satisfactorily explained by the evolutionary programme.

Lakatos's second methodological rule, the positive heuristic, states what paths to pursue. That is, it outlines the future progress of the research programme, in terms of problems to be solved and areas to be investigated. Lakatos, like Popper, places emphasis here on novel predictions. Indeed, a successful research programme should at some time in its history provide us with a prediction of some previously unexpected pattern of findings. The success of Einstein's special theory of relativity, for example, was greatly enhanced (in the eyes of fellow scientists, if not Einstein himself) by the successful prediction of a gravitational red shift and the deflection of light 1.7 seconds of arc for rays of starlight grazing the sun.

For Lakatos, research programmes are never to be entirely rejected; instead they can be either progressive or degenerating. Moreover, a research programme can progress conceptually without necessarily making empirical progress. Commentators on Lakatos have noted that the distinction between progressive and degenerating research programmes is entirely a historical question and provides little help in evaluating currently competing research programmes. After all, one can never be sure just when a previously degenerating programme might afford us with some outstanding new prediction. The notion of an inviolable hard-core has also received some criticism. Laudan (1977) for one notes that the hard-core of a research programme is also modifiable over time. Furthermore, it can be hard to say just when these modifications are such as to suggest that the research programme has changed.

Despite these criticisms, I think that we can hold on to the general framework that Lakatos provides. The hard core of a research programme can be treated as *virtually* sacrosanct, in that attempts to account for anomalous phenomena should be made initially in other parts of the research programme. At times there may be modifications to the hard-core; however, this can be viewed more generally as part of the evolutionary progression of a research programme, rather than as a signal of an old programme's demise and the emergence of a new one.

Comparisons do need to be made between competing research programmes in our own lifetime and I think this can be done by employing the criteria for theory evaluation that I will outline below. These criteria provide us with a guide about what research programme it is best to pursue at any given point in time. However, that does not mean that it is irrational to pursue less acceptable research programmes, given that they provide some hope for future development.

Similar in many ways to Kuhn's paradigms and Lakatos's research programmes, Hooker (1975) provides us with another characterisation of large scale theoretical entities, which he simply calls global theories or theoretical world views. However, Hooker's global theories are more broad in their scope than Lakatosian research programmes and more approximate the global usage of paradigm employed by Kuhn.

> . . . the fundamental intellectual entity in science is the *theoretical-world-view;* this is something like a tightly interconnected ('coherent') set of conceptual categories for grasping the world, finding expression at the most general level as a systematic metaphysics (systematic ontology) then a more particular application as a fundamental theory (standard sense), next as a forging link to other important theoretical areas as necessary. . .
> (Hooker, 1975 p. 155).

Hooker's notion of global theory, therefore, has both an internal and external character. The internal globalness of a theory is represented by a coherent collection of conceptual categories, methodological rules, measuring instruments and so forth. The external globalness of a theory refers to its connections with other theories and aspects of life. At no level of analysis can theories be viewed in isolation. The theoretical world view that science affords us should be coherent both within and between the boundary divisions that are typically employed. Our best theories of sub-atomic phenomena should be compatible with our best theories of atomic, chemical, and neural phenomena. Our best theories of the distribution and evolution of species, should be coherent with our theories of geological and atmospheric change, and so forth. In evaluating global theories this inter-disciplinary coherence must be taken into account.

The two factors of large-scale theory collections that emerge in all three accounts presented here, are that it is these global theories that should be the fundamental unit of cognitive significance and that theoretical pluralism at all levels of theory analysis is fundamental to the growth of science. The history of science counsels us that progress is obtained through the development of multiple scientific research programmes or global theories. Certainly pluralism at this level is crucial for scientific progress. However, typically speaking, pluralism is most readily manifest *within* scientific research programmes. It is the proliferation of theories here that provides a research programme with its progressive character. As new theories are proposed, accepted, and rejected, a research programme increases its explanatory power in providing us with a better population of models that represent the world.

The terminology that I will employ throughout this thesis is that of Lakatos's research programmes, and I will accept the basic framework of his approach. However, I think that it is important to place research programmes in a wider context, one that includes relations to other research programmes, technology and society. So in evaluating research programmes we must take into account not only their internal coherence, but also their relation to other areas of science and to society in general. What follows is a brief characterisation of the way that I shall deploy the notion of research programmes in this thesis. My model of research programmes, which I dub 'Global research programmes' (GRP's) is essentially a hybrid of Lakatos's and Hooker's models of large-scale theoretical entities. Figure one describes in schematic form the general features of GRP's.

Features of Global Research Programmes (GRPs) in science.

Internal characteristics

1. The hard core

This part of the global research programme (GRP) contains the central theories and laws which describe, predict, and explain, in conjunction with auxiliary theories, the relevant phenomena in the domain of inquiry. The hard core is *virtually* immune from falsification; although it can be revised over time, its fundamental features cannot be radically changed without altering the character of the research programme.

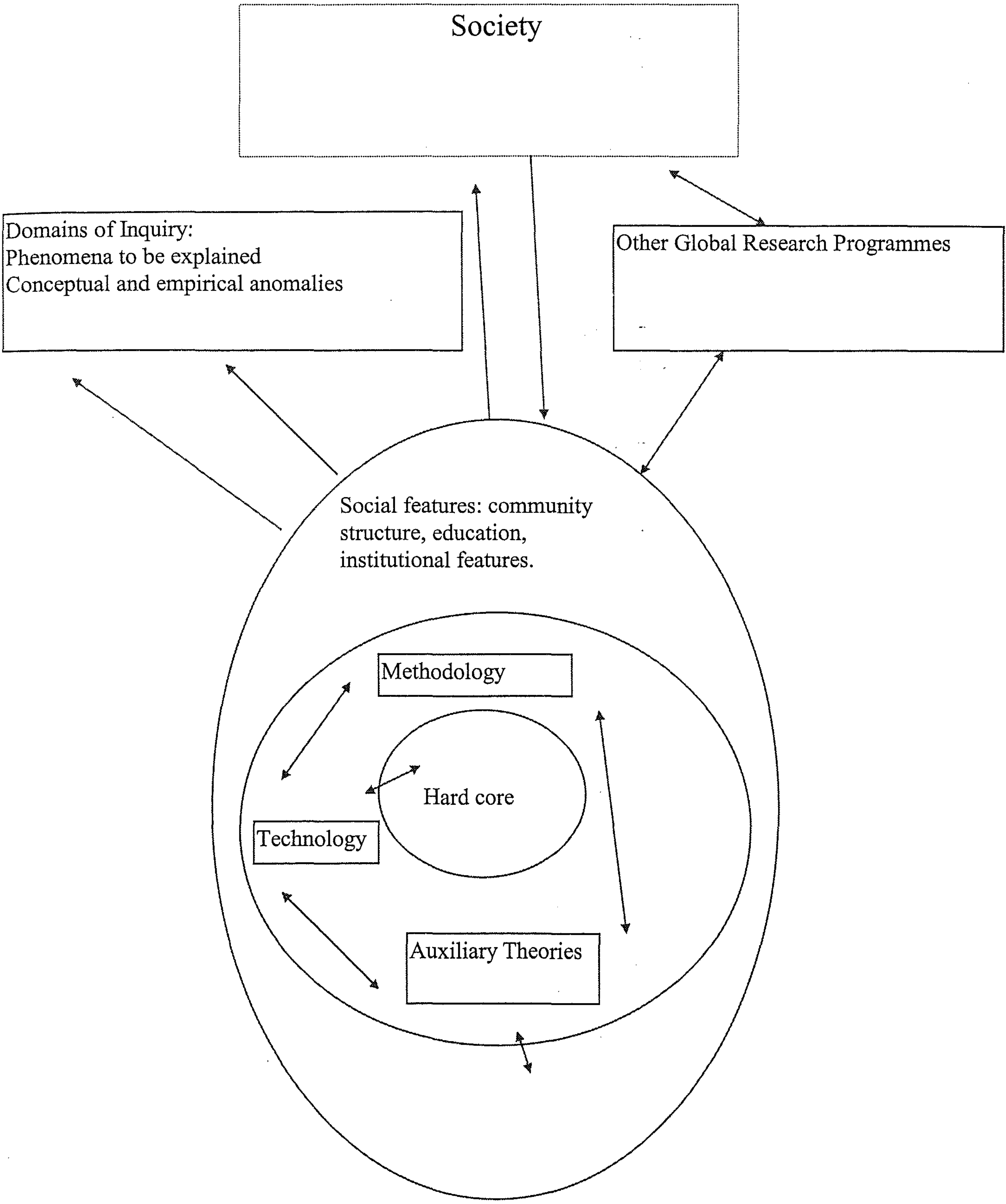
2. The protective belt

The protective belt of the research programme is comprised of auxiliary theories, methodologies, and experimental technologies. If there are specific anomalies confronting a research programme, modifications will be initially sought in the protective belt before the central hard core assumptions are questioned. These modifications, however, should be content increasing and coherent with other features of the research programme.

2.1 Auxiliary theories

The auxiliary theories in a research programme provide a means to extend the explanatory scope and precision of the theories and laws outlined in the hard core. They are often formulated in response to specific anomalies or problems encountered by the research programme, and are typically couched in less general terms than those theories in the hard core. The auxiliary theories of a research programme should cohere both with other auxiliary theories and with the central assumptions of the hard core.

Figure One. The structure of global research programmes in science.



2.2 Methodologies

Typically speaking, a scientific research programme will have an array of various methodologies. Some of these might be specific to the programme in question, while others will be shared with other programmes. Problems in the development of a research programme may, in part, be the result of inadequate or inappropriate methodologies. Likewise, the success of a research programme is in part a consequence of the kind of methodology employed. It is important that the methodologies used are adequate to the task of identifying phenomena, and constructing and evaluating the relevant theories, including the theoretical underpinnings of the methodologies themselves.

2.3 Experimental technologies

In their interactions with the world, scientists will employ a variety of procedures which depend on the appropriate use of technology. Like the methodologies employed, this equipment may be specific to the research programme in question, or a shared part of the wider scientific community. The development and use of technology is reciprocally related to the identification of phenomena, and the construction of theory. Advances in theory will enable commensurate development of technology which may lead to the identification of new phenomena and new means of evaluating theory.

3. *Domains of intellectual inquiry*

Most research programmes in science will embrace a wide variety of domains at different levels of structural organisation. However, scientific research programmes, as they have been developed so far in the history of science, have a circumscribed range of inquiry. For example, although the evolutionary research programme explains a wide range of phenomena, from the functional properties of cells to the dynamics of ecosystems, it is uninformative on the micro-properties of matter or the structure of planetary systems. An important goal of research programmes is the identification of phenomena, in the sense of robust, replicable empirical findings. This elucidation of phenomena evolves in tandem with theory, method and technology as suggested above.

4. *Social features*

4.1 The scientific community

An important part of any GRP is the scientific community which operates within the programme's intellectual framework. This community, in tandem with written sources, contains the living embodiment of the knowledge structures of the programme in question. The nature of the social relations between individuals and the way cognitive resources are divided and deployed can play an important part in the degree and nature of a programme's progress. Ideally there should be a free and open exchange of information regarding all aspects of a GRP. Individuals should be aware of developments not only in their own field of specialisation, but also of more global issues relating to theory, methodology, technology and the programme's multiple interfaces with the rest of science, and more generally speaking, with society.

4.2 Institutional features

The role of institutions in scientific inquiry has often been neglected. However, I consider the institutional features typical of a GRP play a potentially important part in the success or failure of a programme. The way resources are allocated, work reviewed, publications assessed, and rewards allocated, among other things, may substantially influence the kind of inquiry carried out. The structure of academic institutions, of course, does not comply neatly with the boundaries of GRP's. Often members of competing programmes may be working in the same department, or adherents of the same programme may be distributed across different departments. The institutional features of a GRP then, are more likely to be a consequence of general institutional aspects of academic life, although there may also be specific institutional characteristics specific to the programme itself.

4.3 Education

The way scientists are educated also plays an important role in the development and progress of GRP's. Both the style and content of education can influence the emerging community of scientists. Ideally the education of scientists should be concerned with the key features of the GRP itself. This will involve not only the acquiring of the relevant methodologies and facts in the programme's domain, but also should involve a consideration of theoretical issues at various levels of specificity. A familiarisation with the history of the GRP should be an important part of this process. Although the attainment of expertise in narrow domains is a necessary feature of contemporary science education, this fact should not preclude the importance of understanding the relationships between various sub-domains of inquiry and of considering the broader social ramifications of the GRP in general.

External characteristics

1. *Relations to other research programmes*

As Hooker (1975) has emphasised, it is important that the theories, methodologies and so forth, in a given GRP be compatible with those of other GRP's. Furthermore, it will often be the case that the domain of interest will overlap with various different GRP's. The development of appropriate connections between GRP's of various degrees of globalness, therefore, is an important consideration when conducting research and articulating theory. Moreover, progress may be achieved in a research programme by the relevant combination of theories, technologies and the like, developed in different programmes.

2. *Technological applications*

The progress of theory in science and the elucidation of phenomena can not be insulated form the various technological applications that may be employed as a consequence of theory development. Although some scientists may be engaged in 'pure' research, typically speaking scientific research will have some implications for the way humans intervene in the world to further their various goals. This relationship between science and technology is a reciprocal one. The articulation of theory and the construction of scientific explanation play an important role in guiding technology, while technology opens up new ways of interacting with the world so as to provide increasingly richer descriptions of reality.

3. Ethical and political implications

Because the goal of science can be characterised as the production of valuable knowledge (Maxwell, 1984) and because the products of science and the pronouncement of scientists wield considerable impact for the development of society, it is important to recognise the moral implications of GRP's. Science, therefore, cannot be insulated from valuational considerations, and more particularly, from ethical questions relating to the impact of science on society. This relationship between science and society, as with technology and society, is reciprocal in nature. The development and promulgation of scientific ideas can exert a profound impact on social policy, while political considerations may, to a certain extent, direct research and the way that it is subsequently reported. It follows that one way in which a GRP might be evaluated is with respect to the potential social consequences that it implies.

Progress in a research programme can occur in any aspect of the overall structure, be it theoretical, methodological, empirical, institutional, ethical or whatever. Moreover, some research programmes might progress through the forging of appropriate links to other areas of science, or in ways of explaining new intellectual domains. Ultimately, the evaluation of a research programme is made in terms of its overall coherence in relation to alternative programmes. A central aspect of this evaluation will be in terms of the global excellence of theory displayed by the programme in question.

Theory appraisal in science

The account of science that I have presented thus far, suggests that progress is achieved through the succession of increasingly better research programmes over time. However, on what basis should we accept that one theory or research programme is better than another? That this task is even possible at all has been questioned by some, especially those of relativist persuasion. However, the realist account of science that I have defended above suggests that fruitful comparisons *can* be make between alternative research programmes, and decisions can be made regarding their comparative worth.

The evaluation of competing hypotheses is not only an integral part of science, but is also ubiquitous in many other domains, such as legal reasoning and the reasoning processes of lay individuals (Josephson & Josephson, 1994). This kind of reasoning is based on abductive processes. On the discovery of some interesting pattern of data in the world, there is a natural press for an *explanation* of this data pattern. Typically, a range of plausible candidates are suggested. At a later stage, these various candidate hypotheses are evaluated with respect to their overall acceptability. This second stage, whereby more mature theories are evaluated, has been termed 'inference to the best explanation' (Harman, 1964).[12] The important point to note here is that abduction and inference to the best explanation are ampliative and explanatory in nature. They go beyond the data to suggest typically, but not always, hidden processes that serve as explanations of the patterns observed.

[12] There is some debate in the literature as to whether abduction and inference to the best explanation can be considered as separate processes. Josephson and Josephson (1994) retain this distinction, however Thagard (1988) argues that it can not be sustained. Certainly the initial development of plausible explanatory hypotheses must involve some kind of evaluation to avoid a combinatorial explosion of possibilities. However, I think the distinction can still, albeit more fuzzily, be drawn. Abduction can be said to refer to an initial proliferation of *plausible* alternatives, typically articulated by an individual or a number of individuals, whereas inference to the best explanation is reserved to refer to the evaluation of relatively well developed and coherent hypotheses in a more explicitly social and institutional context.

Typically speaking, most theorists have posited a range of criteria from which to judge the adequacy of competing theories. Kuhn (1977) for example, suggests that a good theory should have five major attributes:

(1) It should be accurate. That is, it should fit with the data.

(2) It should be consistent, both internally and with other accepted theories in related aspects of nature.

(3) It should have broad scope.

(4) It should be simple, in terms of bringing order to phenomena.

(5) It should be fruitful. It should lead to the discovery of new phenomena or previously unknown relationships between phenomena.

Just how these criteria are to be weighed against one another is an area for further debate. McMullin (1993) argues that predictive accuracy and explanatory power are to considered primary virtues in guiding theory choice, with the other epistemic values as *means* to these ends. Others (e.g., Lakatos, 1970; Chalmers, 1990) have emphasised the importance of fertility. A good research programme is a progressive one. It is one that leads to the prediction of new results and novel phenomena.

Despite the differences in accounts of theory choice, one theme that seems to emerge as central to theory appraisal is that of explanatory scope or breadth in tandem with simplicity of theory. All other things being equal, a theory or research programme will be considered superior to its rivals if it furnishes us with an explanation of a wide range of phenomena using a minimal number of explanation types or argument patterns (Friedman, 1974; Kitcher, 1981). " Science increases our understanding of the world by reducing the total number of independent phenomena that we have to accept as ultimate, or given." (Friedman, 1974 p. 15).

In both the Newtonian and Darwinian programmes, for example, a small number of explanatory patterns can be used to unify a large number of (apparently) diverse phenomena. Thus, the process of natural selection coupled with the laws of descent can be used to explain a multiplicity of biological phenomena. We can explain social grooming among vervet monkeys, the diversity of beaks on Darwin's finches and the distribution of Southern beech trees, by instantiating these few argument patterns. Evolutionary theory has obvious virtues over three separate kinds of theories used to explain these biological phenomena.

From a realist perspective, focus on unification as central to theory appraisal, suggests that over time theories that fasten on to local or accidental features of a portion of the world are gradually weaned out in favour of approaches which carve reality at its joints.[13]Science, from this perspective, is a search for invariances, which furnish us with increased understanding of the nature of reality.

In terms of theory appraisal, the idea of explanation as unification has many affinities with the model of explanatory coherence developed in some depth by Paul Thagard (1978, 1989 ,1992). Thagard's (1992) model contains seven principles, which serve to provide a means by which to evaluate the epistemic worth of competing hypotheses. Research programmes or systems of explanations are to be evaluated in terms of their overall *explanatory coherence.*

The first principle, symmetry, asserts that the coherence of two propositions in an explanatory system is symmetrical. If propositions P and Q cohere then so do propositions Q and P. The second, and most important principle, is that of explanation. The principle of explanation comes in three parts. The first part asserts that if a hypothesis P explains some patterns of data then P and the data cohere. It follows here that the more a proposition explains the more coherent it is. Thus explanatory breadth, or unification, is an integral part of Thagard's model. The second aspect of explanation states that if a hypothesis P is explained by another hypothesis Z, then P and Z cohere. Scientific research programmes are, of course, rich collections of such internally coherent

[13] Of course it is entirely possible that such a carving is beyond the scope of our or anyone's competence. However, despite the inherent messiness of our blade work, the notion of the increasing unification of theories provides some hope that we are getting closer to true explanations of the world.

explanatory networks. The last aspect of the principle of explanation asserts that the degree of coherence of some explanatory system is inversely proportional to the number of propositions that it contains. This claim embodies the notion of simplicity as it is typically deployed. We should be wary of explanatory schemes which contain too many special assumptions.

The third principle presented by Thagard is that of analogy. If a proposition P explains some phenomenon Q, and another proposition P_1 which is analogous to P explains a phenomenon Q_1, then P and P_1 and Q and Q_1 cohere. The role of analogy in theory development and appraisal, as Thagard (1992) notes, in clearly illustrated in the case of evolutionary theory. Darwin explicitly drew on the evidence from artificial selection to support the validity of his proposed mechanism for evolution, natural selection.

The fourth principle is that of data priority. Propositions that describe patterns of data have some degree of acceptability on their own. Principle five, contradiction, asserts simply enough, that propositions which contradict one another incohere. This principle reflects the importance of consistency in any explanatory scheme. If two propositions which are not explanatorily connected both explain a certain class of phenomena they are said to incohere. Thus competition, Thagard's sixth principle, is seen as an important part of theory appraisal. Often, but not always, alternative explanatory schemes will compete with one another as the best explanation for a given range of phenomena.

The final principle, acceptability, has two parts. The first part states that the acceptability of any proposition in an explanatory scheme depends on its degree of coherence with other propositions in that system. The second part asserts that the acceptability of a proposition is mitigated by the number of unexplained relevant phenomena. A proposition which successfully accounts for a few relevant phenomena, while not explaining many others, will be deemed as less acceptable.

These principles developed by Thagard seem to incorporate many of the criteria suggested by other theorists as integral to theory appraisal. The acceptability of Thagard's model, however, is itself dependent on the explanatory coherence of his approach in relation to alternatives. To evaluate his model Thagard has implemented the seven principles of explanatory coherence in a connectionist network, ECHO, and more recently ECHO2. These are computer programmes which serve to model cases of theory choice between competing scientific research programmes. If the theory of explanatory coherence provides a good explanation of theory choice in science then the results of running ECHO on important historical episodes in science should conform to actual changes in dominant research programmes.

The results of the simulations run by Thagard (1992) provide considerable support for the theory of explanatory coherence. The replacement of phlogiston theory by oxygen theory, the acceptance of Darwinian theory, and the revolutions in physics and geology, are all explained in terms of the greater explanatory coherence of the successful research programmes over their competitors.

The theory of evolution, for example, uses three main hypotheses: Organic beings are in a struggle for existence, organic beings undergo natural selection, and species of organic beings have evolved, to explain a wide variety of natural phenomena. Darwinian theory explains the complexity of animal behaviour and physiology, species extinction, the geographical distribution of species, the similarities in morphology and development between species and so on, by employing these small number of principal hypotheses. Evolutionary theory evinces both greater explanatory breadth and simplicity than does its main competitor, creationism. Darwin's theory also draws support from the analogy with artificial selection. If animals can be modified over time through selective breeding, then modification can also occur through natural selection. In running ECHO on the comparative worth of Darwinism and Creationism, Darwinian theory clearly emerges as the more explanatorily coherent theory.

The analysis of science afforded by Thagard's theory of explanatory coherence suggests that the kinds of epistemic values used to evaluate research programmes remain, broadly speaking, invariant across the history of science. I say *broadly* speaking, because an analysis of individual scientists or even some specific scientific episodes, may reveal considerably more variability. As Kuhn (1977) argues, the fixity of epistemic values does not preclude a considerable flexibility in terms of how they are deployed in any given context.

A naturalistic approach to science emphasises the nature of science as a human activity. Scientists are decision makers (Giere, 1988), employing some combination of the principles delineated by Thagard, in service of their goals, both as scientists and more generally as human beings. Individual scientists will vary in many respects. For a start, they will have different relationships with the various relevant bodies of evidence. Their education, although probably similar, will also vary in terms of emphasis and direction. As Kitcher (1993) points out, in thinking of evolutionary problems some of the great biologists have been considerably influenced by their own experiences and predilections. Darwin himself was guided by his experiences on the Beagle, his interest in artificial breeding and his reading of Malthus. The framing of evolutionary questions for E. O. Wilson, on the other hand, has been strongly influenced by his passion for ants, and more latterly by his interest in conservation.

The intellectual profile of scientists will also reveal differences in terms of fundamental cognitive strategies (Kitcher, 1993). In the language of Gardner's (1983) theory of multiple intelligences, some scientists will be more richly endowed with visuo-spatial or logical-mathematical intelligence and will approach problems and frame solutions in different terms than those who are more linguistically orientated. Scientists will also differ with respect to their social and political allegiances. However, despite these differences, Thagard (1992) has argued that social and political factors rarely play a direct role in the acceptance and rejection of theories. The case studies presented by Thagard provide no indication that extra-scientific motivational factors play an important part in major episodes of scientific change, although undoubtedly these factors can exert some influence on individual decision-making processes. Indeed, as Thagard (1992) notes, any individual socio-political predilections tend to be mitigated by the socio-institutional processes of organised science.

> Because of an institutional commitment of science to experimental evidence and explanatory argument, science as a whole is able to transcend the personal goals of its fully human practitioners who acquire the motivations to do good experiments and defend them by rational argument.
> (Thagard, 1992 p.113).

This is not to deny the potential importance of other, external factors, in the acceptability of a theory or research programme. Consistent with the model of GRP's outlined earlier, the social implications of a theory will also play some role in its acceptance or rejection. For example, if a theory has potentially important political or social ramifications our standards for theory evaluation may be higher. Conversely, if a particular theory suggests important and socially relevant courses of action we may be more ready to accept it as a reasonable theory. Haig (1989) for example, has persuasively argued that questions of risk ought to play an important role in the appraisal of scientific theories. In assessing the adequacy of nuclear winter theory, for example, we should not only employ those values typically conceived of as legitimate, such as simplicity, explanatory breadth and so on, but we should also consider the costs of being wrong. In the case of nuclear winter theory the risk in downplaying the effects of a thermo-nuclear war, that is, in rejecting nuclear winter theory, are potentially disastrous. In contrast, by accepting the theory (although it hasn't been 'proven'), we encourage the down-scaling of nuclear weapons below a critical threshold level and hence reduce the possibility of a nuclear winter occurring with its globally catastrophic effects. As Haig makes clear, we can no longer accept the empirical orthodoxy that the aim of science is the pursuit of near-certain value-free knowledge. If we are to do justice to the goals of discovering and realising what is of value in existence then we must accept the role that external factors have to play in the development of science.

The heterogeneity of socio-cognitive profiles revealed by individual scientists will be reflected in the way questions of explanatory coherence are to be evaluated in specific cases. For example, Giere (1988) has suggested that one of the reasons why Wegener's theory of continental drift was originally rejected was due to the overwhelming majority of influential Northern Hemisphere scientists in evaluating Wegener's theory. The evidence for continental drift is strongest in the Southern Hemisphere, where comparative analyses of floral and faunal profiles provide strong evidence for the connection of the various Southern Hemisphere landmasses in one super-continent, at some time in history. At the time that Wegener proposed continental drift, stabilism may well have been the most explanatory coherent theory. However, if the evidence from the Southern Hemisphere studies had been more salient, or more visible to the major geologists of the time, mobilism may have provided the most explanatory coherent alternative.

At all times, of course, scientists can only choose the best theory which is available to them. Even if that theory is fundamentally wrong, it is rational to pursue it in the absence of better alternatives. Like the movement of a species across an adaptive landscape, scientists can only explore the local optima of theory available to them, although there may be much better alternatives inaccessible at other locations.

In providing a rich and informative account of theory appraisal, Thagard's theory of explanatory coherence needs to be understood at both the relatively more coarse-grained level of major changes in the history of science and also in terms of the individual decision-making processes of scientists. Thagard's theory also provides, more generally speaking, an understanding of decision-making processes in non-scientific arenas. An evolutionary approach to theory appraisal focuses on the adaptive value of theory-choice across different domains and in different species.

Relationships between theories

If, as many have suggested, theoretical pluralism is essential for the growth of science, then pair-wise evaluations of theories or research programmes is, or should be, an ongoing activity in science. However, research programmes are not necessarily in direct conflict with one another, even if they purport to provide explanations of the same range of phenomena. The cognitive relationships between theories can vary in terms of the epistemic conflict or threat that one theory poses for another.

Two useful schemes for evaluating the conceptual relationships between theories have been presented by Laudan (1977) and Thagard (1992). If a new theory T_1 completely absorbs or entails a previous theory T_2, then T_1 can be said to *incorporate* T_2 (Thagard, 1992), or be considered to be in a relationship of *entailment* (Laudan, 1977). If T_1 only partially incorporates or provides a rationale for T_2, while rejecting parts of the theory, then T_1 can be said to *sublate* (Thagard, 1992) or *reinforce* (Laudan, 1977) T_2. If, however, T_1 invokes the near total rejection of T_2, then T_1 can be said to *supplant* T_2 (Thagard, 1992). This context is similar to Laudan's conditions of *implausibility* and *inconsistency* where T_1 either entails that T_2 is *unlikely* or entails the negation of (most of) T_2. If a new theory simply ignores an older theory, then for Thagard (1992) they can be said to *disregard* one another. This may occur for any number of reasons. Theories may also be *compatible* if T_1 entails nothing at all about T_2 (Laudan, 1977).

This conceptual scheme for evaluating the relationships between theories can help to clear up many problems encountered in arguments regarding the comparative worth of theories. In some cases proponents for a particular approach vigorously debate the merits of an alternative perspective without considering how the two approaches may be related. Some scientists who actively oppose evolutionary explanations in the social sciences, for example, often wrongly suggest that evolutionary and social explanations are inconsistent or implausibly related to one another. The conceptual error here is twofold. First of all, a scientific research programme, evolutionism, is being contrasted with a *domain* of inquiry, social behaviour; there is no straight forward pair-wise evaluation of theory. Secondly, it is assumed that evolutionists envisage the relationship as one of supplantation, when it is, I believe, better conceived of as sublation. Evolutionary theory incorporates (partly) rather than rejects social theory.

The nature of scientific explanations

One important function of global research programmes in general and theories more particularly, is to provide explanations. We say that the core assumptions of the evolutionary research programme, plus the relevant auxiliary theories, furnish us with explanations of biological phenomena. But what does it mean to say that something has been explained? A popular answer to this question is to suggest that explanations provide us with an increased *understanding* of the world (Friedman, 1974; Kitcher, 1981; Salmon, 1989). Science tells us not only *that*, but also *why*. That is, science provides us not only with descriptive knowledge of the way the world is, but also explanatory knowledge; it tells us *why* the world is the way that it is. But just how do explanations serve this goal of furthering our understanding of the world?

The received view

Although there is a general consensus regarding its inadequacy, or at least incompleteness, as a theory of explanation (see Kitcher & Salmon, 1989), the most important and influential account of scientific explanation was that provided by the philosopher Carl Hempel (e.g. 1966). For Hempel, to explain something was to provide an argument to the effect that the thing to be explained was to be expected given certain antecedent conditions, coupled (usually) with some laws of nature. A central form of scientific explanation for Hempel comprised the so-called deductive-nomological explanations. These kinds of explanations, as their name implies, are deductive in nature with the conclusion or *explanandum* sentence being derived from the premises or *explanans* sentences, in which there must figure at least one scientific law. Schematically:

$L_1, L_2, \ldots, L_p$ } Explanans (laws)
$C_1, C_2, \ldots, C_k$ } sentences (initial conditions)

E Explanandum sentence

For example, Leverrier explained the irregularities in the motion of Uranus (the explanandum) by invoking Newton's law of gravitation and postulating an, as yet unknown, planet of a particular size, position, and mass (the explanans) (Hempel, 1966).

Hempel also provided an account of probabilistic explanations. In some cases the explanandum will not follow with deductive certainty from the premises, but only with a high degree of probability. The pattern of the argument, however, is similar to that of deductive-nomological explanations.[14] For example:

The probability for persons exposed to the measles to catch the disease is high.
Jim was exposed to the measles.

-- [Makes highly probable that]

Jim caught the measles.

Kitcher (1989, p 410/411) summarises the key features of Hempel's account of explanation as: (1) The idea that explanations are arguments; (2) The conclusion of an argument is a sentence describing the phenomenon; and (3) That the premises of an explanation must contain at least one law.

[14] Hempel (1966) also provided an account of inductive-statistical explanations, however, this figures less prominently in his overall model of scientific explanation.

Problems with the received view

Although widely influential and seemingly capable of providing an adequate account of many explanations in science, Hempel's model of explanation has accumulated over the years a considerable body of criticism from a diversity of sources. These criticisms are summarised by Kitcher (1989) and Salmon (1989).

Firstly, there are many accounts in science which appear to be explanatory in nature, but do not fit the deductive-nomological model. For example, the narrative style of explanation found in history and sciences such as evolutionary biology and palaeontology appear to provide us with satisfactory explanations of phenomena, but do not typically invoke laws, and do not conform to the pattern of deductive arguments.

Conversely, there are also arguments which fit the pattern of the deductive-nomological model but do not provide us with adequate explanations. To use the standard example:

	A) The shadow of a flagpole.
coupled with	B) The elevation of the sun and the laws of the propagation of light.
	————————————————
does not explain	C) The length of the flagpole.

If we exchange C for A in the example above, however, we have provided a reasonable explanation. The problem here is one of causation. It is the height of the flagpole coupled with the relevant laws and initial conditions which causes, and hence explains, the length of the shadow; not the shadow which causes (and therefore explains) the length of the flagpole (Salmon, 1989). The co-occurrence of sharp drops in barometer readings and the commencement of storms provide a similar counter example. Whenever barometric pressure drops sharply a storm will occur. However, the drop in the reading does not cause, and hence fails to explain, the presence of the storm. The barometer reading and the storm are the effects of a *common cause*: atmospheric conditions of a certain kind. So although the explanation of the storm on the basis of barometric readings conforms to the deductive-nomological model, it is not truly explanatory in nature.

Another problem for Hempel's conception of scientific explanation is that of *explanatory relevance*. The problem is illustrated with the example of the hexed salt. A magician waves his wand over a sample of salt which has been placed in water. The salt subsequently dissolves. What explains this fact? An explanation which conforms to the deductive-nomological model could be constructed whereby the dissolved salt is explained by reference to the law that 'all hexed salt dissolves in water' and that this salt was so hexed. Clearly the invocation of hexing is non-explanatory in this context. Similarly, we do not explain Bob's failure to conceive a child by reference to the fact he consumes on a regular basis his wife's birth-control pills, even though it is true that all males that ingest female contraceptives fail to become pregnant (Salmon, 1989). The problem here is that arguments that conform to the deductive-nomological model of explanation do not necessarily pick out the *relevant* features from the irrelevant ones in furnishing us with explanations of phenomena.

Finally, Hempel's account of explanation has difficulty in providing an understanding of very rare events, even though they may be furnished with adequate explanations. The favourite example here is the case of the mayor's contraction of paresis. We can explain this fact by reference to the mayor's previous bout of syphilis, and noting that occasionally syphilitics contract paresis. The previous incidence of syphilis explains the subsequent contraction of paresis even though the probability of this occurrence is extremely low. What is important in this and similar cases, Salmon (1989) emphasises, is not high probability but statistical *relevance*. It is important that the facts cited make a *difference*.

This brief tour of Hempel's account of explanation and some of the problems that have been directed against it fails to do full justice to the complexity of the issues involved. However, my aim here has been to highlight those aspects of explanation that any fully realised approach to explanation must take into account. It is important in this context, I believe, that our account of explanation be global in nature. That is, the best theory of explanation should be one that not only makes sense of explanations in physics or the physical sciences, but also in biology, psychology, and the social sciences. The account of scientific explanation that I favour here follows from a brief, but lucid and influential article, by Kitcher (1985), in which two forms of (essentially compatible) explanation are forwarded. The first, *causal-mechanistic* approach, championed by Salmon (1984, 1989) among others, views explanation as an elucidation of causal mechanisms. The second approach, as developed by Friedman (1974) and advanced by Kitcher (1981, 1989), conceives explanation as unification: the derivation of a wide range of phenomena from a small set of underlying argument types. I shall also argue that there are important pragmatic considerations which must be taken into account in any model of scientific explanation.

The causal-mechanistic approach to explanation.

Part of the problem with the deductive-nomological model of explanation, as highlighted by the examples above, is that it fails to adequately address the role of causation in explanation. The subsumption under laws is not enough, we also need an understanding of the causal relationships that obtain between explanandum and explananda. Only then can we make sense of the idea that the height of a flagpole explains the length of the shadow and not vice versa. This *ontic* conception of explanation has been rigorously formulated and championed by Salmon (1984, 1985, 1989) among others (e.g., Railton, 1981; Bunge, 1996). From this perspective, to explain something is to show how it fits into the causal structure of the world. "Explanatory knowledge opens up the black boxes of nature to reveal their inner working. It exhibits the ways in which the things we want to explain come about" (Salmon, 1989, p.182).

The causal-mechanistic approach to explanation focuses on the way explanations tell us how the world works. This typically requires details of the mechanisms which underlie and give rise to empirical regularities. It is only through a detailing of such mechanisms that understanding is achieved (Bunge, 1996). The causal-mechanistic approach to explanation is wedded (although not inevitably) to a realist view of nature. Because much of the interesting furniture of the world is inaccessible to us, explanations often need to advert to unobservables; to the hidden mechanisms which reside in nature. A distinction can be made here between descriptive and explanatory knowledge. Descriptive knowledge is knowledge of appearances. Explanatory knowledge, by contrast, provides an understanding of the underlying mechanisms which produce the phenomena of interest (Salmon, 1989).

From the causal-mechanistic concept of explanation we can explain why Bob did not get pregnant and the mayor contracted paresis by an examination of the relevant mechanistic processes. Human males simply do not possess the necessary physiological mechanisms to enable pregnancy, while the mayor's paresis can be shown to follow from the action of certain causal processes at the biochemical and physiological level of description.

Science progresses, on the causal-mechanistic view of explanation, as more detailed mechanistic hypotheses are elaborated which reach deeper levels of reality (Bunge, 1996). An important and useful distinction in this context is that made by Railton (1981) between *ideal explanatory texts* and *explanatory information*. The ideal explanatory text is an account of all those relationships that obtain, causal or otherwise, over various levels of analysis, which give rise to the phenomenon of interest. Explanatory information, by contrast, refers to specific portions of the ideal explanatory text which are being examined. Given the extraordinary complexity of the world and the myriad kinds of causal relationships that can obtain between phenomena, the ideal explanatory text may be an *unobtainable* ideal. However, this is not important, for science can be seen to advance by seeking out explanatory information which fills out parts of the ideal explanatory text.

A focus on underlying causes can also serve more practical ends. By teasing out the causal relationships which exist in the world we are in a position to intervene in nature so as to bring about desirable results (Salmon, 1989). As Bunge (1996, p. 422) notes:
" There is nothing like the disclosure of mechanisms to destroy myths and to empower us to control natural and social processes."

In summary, the causal-mechanistic approach to explanation suggests that our understanding of the world is advanced by knowing the detailed mechanisms (causal or otherwise) which are responsible for the phenomena that we wish to explain. Reference will often be made, in the filling out of the ideal explanatory text, to processes, entities and relations which are non-observable in nature. Such an approach not only captures what it means to explain why the mayor contracted paresis or why Bob did not get pregnant, but also provides a way of intervening in the world to obtain more pragmatic ends.

Explanation as unification

A somewhat different approach to explanation suggests that explanation can be conceived of as unification (Friedman, 1974; Kitcher, 1981, 1989). Science can be viewed as advancing our understanding of the world, from this perspective, by positing unifying schemes which explain a diversity of phenomena. In this manner we reduce the total number of independent phenomena that we have to accept as given (Friedman, 1974). This approach is what Salmon (1989) terms the *epistemic* conception of explanation.

According to Kitcher (1981, 1989) the sciences draw on a limited set of arguments - the explanatory store - which provide a means of explaining a wide range of phenomena. A theory unifies our beliefs to the extent that it can provide a few general patterns of arguments which can be deployed to explain a large number of facts about the world. Kitcher (1989, p. 432) clearly summarises this perspective on explanation:

> Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as ultimate (or brute).

This perspective on explanation fits in neatly with some of the ideas outlined earlier on theory appraisal. Two important criteria for appraising theories are explanatory breadth and simplicity (parts one and three of Thagard's second principle of explanatory coherence, explanation). *Ceteris paribus,* we should prefer theories which explain a wide range of phenomena without invoking a large number of special assumptions. As Kitcher (1981, 1989) notes, many theories in science are accepted because of their explanatory *promise*. This promise is tied closely with notion of unification. For example, Darwin's theory of evolution was widely accepted *prior* to its detailed confirmation, because it promised to explain a wide range of biological facts, using a few simple argument patterns. Darwin offered explanatory sketches in place of detailed evolutionary stories. He demonstrated how phenomena *could* be explained by implementing certain argument patterns which he had demonstrated in explaining other biological facts (Kitcher, 1989).

The notion of explanation as unification deals effectively with the problems of irrelevance and accidental generalisation. We resist the claim that it was the magician's hexing which explained the salt dissolving in water by appealing to the fact that an alternative theory (one grounded in chemical theory referring to the molecular composition of salt etc.), can explain why *both* hexed and non-hexed salt dissolves in water. That is, the chemical theory better *unifies* the facts that we know about the world. Appeals to hexing on the other hand, only fasten onto restricted portions of the world and hence is limited in scope.

The notion of explanation as unification provides a global means of ordering the world. It furnishes us with a way of understanding the regularities in nature. It also allows us to make sense of developments in the history of science which demonstrate the preference for theories which can order and systematise a wide range of diverse phenomena in a relatively simple manner.

How does this notion of explanation as unification fit in with the idea presented earlier that explanation is concerned with un-covering the causal structure of the world? Importantly, as both Kitcher (1985, 1989) and Salmon (1985, 1989) emphasise, these two approaches to explanation are essentially *compatible* with one another. They provide a complementary means of comprehending how scientific understanding of the world is obtained.

The notion of explanation as unification is a 'top-down' approach (Kitcher, 1985). Its goal is to construct a coherent world picture, viewing particular facts as instances of more general regularities. By contrast, the causal-mechanistic concept of explanation is a 'bottom-up' approach (Kitcher, 1985); it is more concerned with explaining how individual event came about. Clearly both forms of explanations are necessary to advance our scientific understanding of the world. In our attempts to decipher the ideal explanatory text, we need to draw on whatever global theories are on offer and demonstrate how they further our understanding of specific mechanisms which are in operation in the world, and which give rise to the phenomenon of interest.

For example, we may want of explain why some frogs, so paradigmatically amphibious, happen to exist in some extremely arid regions such as the Colorado desert and the Gran Chaco of Paraguay (see McClanahan, Ruibal, & Shoemaker, 1994). One kind of explanation, informed by the top-down approach, would be to draw on evolutionary theory and demonstrate how frogs with certain characteristics under certain environmental conditions out reproduce other frogs. We explain the frog's existence by invoking the notion of biological function and by adverting to the theory of natural selection. However, we also want to explain just *how* the frogs, seemingly suited to life near water, manage to exist under the arid conditions. To do so, we note a number of behavioural and physiological mechanisms which the frogs possess which allow them to conserve water and stay cool. For example, we note that that certain species of frogs and toads stop producing urine when water is not available, and allow wastes to accumulate in body fluids. Dehydrated frogs are also capable of absorbing water much more readily through the skin in a response mediated by a posterior pituitary hormone, arginine vasoticin. In other words, we explain the frogs' presence in the desert by elaborating the detailed mechanisms which enable them to survive under extremely arid conditions; we take a bottom-up approach.

Clearly, both these kinds of explanation are valid and true (as far as they go), and are necessary to advance a complete understanding of the phenomenon of arid dwelling anurans. Just which kind of explanation we may prefer at any given time may be, partly at least, a matter of personal interest. Evolutionary biologists may be more interested in the functional explanation as an instance of natural selection at work and concern themselves with relevant phylogenetic and genetic details of the case, while animal physiologists and behavioural ecologists may be more concerned with the specific details of the behavioural and physiological mechanisms in operation. It is here that the pragmatic features of explanation play a role in determining the kind of explanation that is deemed relevant in different situations.

The pragmatics of explanation

In recent years, the most articulate defender of the pragmatic conception of explanation has been Bas van Fraassen (1977, 1985). For van Fraassen an explanation is an answer to a *why question*. The appropriate answer to the question asked can only be determined by contextual features, such as the background knowledge of the questioner. Explanation is conceived as providing a bit of relevant missing information. Any information can play this role depending on the nature of the question. "Which factors are explanatory is decided not by features of the scientific theory but from concepts brought from outside." (van Fraassen, 1985, p. 324). For example, in answering the question: 'Why did Rebecca drink the glass of beer?', we can provide multiple answers depending on context. For instance, 'Because she was thirsty', or 'because her husband has just left her and she wants to drown her sorrows', or 'because her husband has left her and she wants to celebrate'.

Van Fraassen's notion of explanation places emphasis on the idea of contrast classes. We need to explain why one state of affairs arose rather than another. How an explanation-seeking why question is answered will be determined, in part, by the relevant emphasised contrast class. For example, the question Why did *Rebecca* drink the glass of beer? asks us to provide information which explains why Rebecca and not someone else consumed the beer. Whereas the question, why did Rebecca *drink* the glass of beer? promotes an answer in terms of why Rebecca drank the beer rather than throw it away or pour it over somebody's head.

While it seems clear that pragmatic considerations do play an important role in what we deem as an acceptable explanation, I follow Kitcher (1989) and Salmon (1989) in rejecting the idea that pragmatics is *all* there is to explanation. The important point of departure here is van Fraassen's commitment to a form of empiricism, in contrast to a realist to a realist construal of science favoured by both Kitcher (1989) and Salmon (1989). Explanations, as Kitcher (1981) notes, are not just accepted for their predictive power or empirical adequacy, but are often selected for their explanatory promise in unifying disparate collections of phenomena. It is hard to see how advancement in science could be achieved without reference to unobservable entities and processes, ones which underlie more manifestly pragmatic considerations. Moreover, explanation is not always just a matter of answering why questions; there are also explanation-seeking *how-possibly* questions. *How actually* did mammals get to New Zealand is not the same question as *why* they got there (Salmon, 1989).

One way of reconciling the pragmatic with the ontic and epistemic conceptions of scientific explanation is to deploy Railton's (1981) distinction between explanatory information and the ideal explanatory text (Salmon, 1989). The ideal explanatory text contains all the objective aspects of explanation; it contains all the *relevant* considerations. What *part* of the ideal explanatory text that we want illuminated invokes pragmatic considerations.

> The ideal explanatory text determines what constitutes explanatory information and distinguishes it from explanatory misinformation. *Relevance* is a matter of objective fact; *salience* is a matter of personal or social interest. (Salmon, 1989, p.161).

Salmon (1989, p. 135) provides a useful summary to the three forms of explanation outlined above and how they increase our understanding of the world.

> Our understanding is increased (1) when we obtain knowledge of the hidden mechanisms, causal or other, that produce the phenomena we seek to explain, (2) when our knowledge of the world is so organised that we can comprehend what we know under a smaller number of assumptions than previously, and (3) when we supply missing bits of descriptive knowledge that answer why-questions and remove us from particular sorts of intellectual predicaments.

These three forms of essentially compatible explanation will provide a fruitful means of understanding the role that evolutionary explanations play in psychology as I will detail in chapters five and seven below.

The role of global research programmes in scientific explanation

In the present context it is worth additionally noting the important role that GRP's play in developing satisfactory explanations of relevant phenomena. Firstly, and most importantly, GRP's provide a source of explanatory unification. That is, the hard core of the programme typically provides some very general means of explaining a wide range of diverse phenomena within the domain of the programme. Explanations for specific phenomena draw on the hard core accompanied by the relevant auxiliary theories. Secondly, as the auxiliary theories become increasingly more fine-grained they begin to conform to specific causal-mechanistic explanations of the facts under consideration. Essentially the mechanistic details which give rise to the phenomenon under considerations can be conceived of as *instantiations* of the general argument patterns embodied in the hard core of the programme. Thirdly, given that GRP's contain important social relations, both internal and external in nature, it is reasonable to suggest that pragmatic considerations will also play an important role in the kind of explanatory information that is specifically sought.

For example, in explaining the digestive processes of cows, we can refer simultaneously to the *function* of bovine digestion and the theory of natural selection, as well as to the more or less specific theories of the physiology and biochemistry of digestive mechanisms. Moreover, just what aspect of bovine digestion that we are interested in may be directed in important ways by our specific needs, be they framers wanting to trial new feed types, or environmentalists concerned about the release of methane gas as a by-product of bovine gut activity.

As I see it, GRP's play an important role in specifying the relevant problems and directing research in such a way as to integrate and inform these different ways of understanding scientific explanation. In the filling out of parts of the ideal explanatory text, GRP's specify the general forms of argument which are likely to prove fruitful in illuminating specific mechanisms which give rise to the phenomena that we wish to explain. Because science has important responsibilities to society, just what portions of the ideal text that are elucidated will be directed, to some extent, by purely pragmatic considerations. Ultimately, our goal of understanding the world can be conceived as a striving for the whole truth where partial and valuable truth are of considerable epistemic and moral worth.

Progress in science

Broadly speaking, there are two kinds of change, or from a realist's perspective, progress, in science. Firstly, there is progress that is achieved form *within* a global theory or research programme. Progress here is achieved mainly through accumulation. There is an increase in the number of stable reports of phenomena and explanatory extensions to the scope of the research programme (Kitcher, 1993). Kuhn (1970) characterises this kind of progress in 'normal' science as *puzzle-solving.* Scientists working within a paradigm or research programme provide increasingly better solutions to conceptual and empirical problems throughout time, while the number of anomalies are gradually reduced.

The second kind of progress, and the one that has received the greatest attention is change *across* research programmes. Typically speaking, one research programme is rejected or supplanted by another, better, conceptual scheme. Change in this instance is more dramatic in nature, and in Kuhnian terms represents nothing less than a scientific revolution. The acceptance of Copernican and the rejection of Ptolomeic astronomy, and the widespread acceptance of Wegener's continental drift theory, are two examples of revolutions in science. The *depth* of change that occurs in episodes of scientific revolution, however, varies in different cases.

Figure Two. **Degrees of conceptual change** (Thagard, 1992 p. 35)

1. Adding a new instance, for example that the blob in the distance is a whale.
2. Adding a new weak rule, for example that whales can be found in the Arctic ocean.
3. Adding a new strong rule that plays a frequent role in problem solving and explanation, for example that whales eat sardines.
4. Adding a new part-relation, for example that whales have spleens.
5. Adding a new kind-relation, for example that a dolphin is a kind of whale.
6. Adding a new concept, for example *narwhal.*
7. Collapsing part of a kind-hierarchy, abandoning a previous distinction.
8. Reorganizing hierarchies by *branch jumping,* that is, shifting a concepts from one branch of a hierarchical tree to another.
9. *Tree switching,* that is, changing the organising principle of a hierarchical tree.

Thagard (1992) provides a useful characterisation of the various degrees of conceptual change that may occur (see figure two). Relatively undramatic and frequent changes in a conceptual scheme occur when new instances of a concept or new rules relating to concepts are added to the conceptual repertoire of a research programme. Over the last one hundred years or so, biologists have provided us with a number of such instances, as new species were discovered and aspects of their physiology and behaviour were elaborated. These kinds of changes, typically speaking, do not threaten the conceptual integrity of a research programme.

More dramatic changes occur when previous distinctions between kinds are collapsed or a concept is shifted from one branch of a hierarchical tree to another. For example, Darwin collapsed the distinction between species and varieties (important to creationist accounts) and recategorised humans as a kind of animal rather than a completely different kind of creature. This kind of branch-jumping is a hallmark of all major scientific revolutions (Thagard, 1992). The most dramatic kind of conceptual change is that of tree switching, where the whole basis for hierarchical organisation is altered. Darwin for example, altered the classification of species from one based of similarity to one tied to historical descent (Thagard, 1992).[15]

These changes in conceptual schemes are accepted because of the increase in explanatory coherence that they afford us. Conceptual progress, therefore, both within and especially across research programmes can be characterised, from a realist perspective, as adjustments to our explanatory and categorisation schemes which better represent the true nature of the world. The history of science can be conceived in terms of increasingly more realistic orderings of nature.

Other kinds of progress in science also occur, which are variously connected to conceptual progress. Explanatory progress, for example, " consists in improving our view of the dependencies of phenomena" (Kitcher, 1993 p.105). Often the scope of a research programme will be expanded on over time to include new phenomena, while the explanations of previously isolated phenomena will be refined and elaborated. The Darwinian research programme for example, has over time, furnished us with more satisfactory (explanatory coherent) explanations of phenomena, such as altruism, while extending its scope to incorporate newly discovered phenomena such as the structure of DNA and the social framework of primate societies. Explanatory progress, as in these Darwinian examples, is often the result of changes to the theoretical structure of the research programme. The focus on the gene as the level of selection (Williams, 1966) and subsequent refinements to the theory of kin altruism (Hamilton, 1964a, 1964b), provided better explanations of the seemingly altruistic behaviour of many animals, in particular the eusocial insects.

[15] The implications of some of these changes are yet to be fully realised. In ethics, for example, typically humans still appear to occupy a unique position apart from other animals. Even in taxonomy, recent findings from molecular dating techniques (Sibley & Ahlquist, 1984; Hasegawa, Kishino & Yano, 1989) indicate that humans, chimpanzee and (possibly) gorillas, should, historically speaking, be considered as one genus.

Organisational progress occurs in a research programme when there are improvements in the relations between different areas of science (Kitcher, 1993). Change in science often occurs through the merging or blending of different scientific fields. These sorts of changes can be seen as improving both the internal and external coherence of a scientific research programme.

Progress can also be achieved in instrumental terms (Kitcher, 1993). New techniques and experimental methods contribute to the progressiveness of a research programme by providing access to previously unexplored portions of the world (and therefore revealing new phenomena), and by refining methods of confirmation and disconfirmation. In purely pragmatic terms, a greater explanatory understanding of the world also provides new means of intervening in reality to provide (theoretically at least) material benefits to humans and other animals.

Summary

If the goal of science is to provide us with significant or valuable knowledge about the world, then a realist construal of theory and theory appraisal provides us with a means of evaluating the instantiation of this goal. Wherever possible we should pursue research programmes which provide us with an increased understanding of the rich and complex dependencies of nature in both its observable and non-observable manifestations. In other words we should seek conceptual schemes which have greater explanatory coherence than the relevant alternatives. This increase in understanding provides us, not only with epistemic food for our explanatory hunger, but also with a richer understanding of the ways we can interact with nature to promote that which is of most value in existence.

Many of the themes presented in this chapter will be used in my analysis of the role of evolutionary explanations in psychology in subsequent chapters. In particular, I focus on the relations between evolutionary theory and other research programmes in psychology and on the relative degree of explanatory coherence that they offer. In the next chapter I look at some of the more general issues regarding the role of theory in psychology and the place of psychology in science, generally speaking.

Chapter two

The nature of psychology

Much confusion has been engendered over the disciplinary status of psychology. Should psychology be considered a proto-science, a fully-fledged science, a radically different kind of science, or not a science at all? What sort of explanations should we be seeking in psychology and what sort of strategies do we need to employ to further our understanding of psychological phenomena? The lack of a clear answer to these questions is due, I shall maintain, to widespread disagreement over both the nature of science and of psychology.

In this chapter I will argue that psychology *should* be considered a science. Moreover, although psychology, like all areas of academic inquiry, has unique characteristics, there is no radical discontinuity between psychology and the natural and social sciences. As such, psychology as a science can be understood, from a naturalist-realist perspective, as part of a diverse but coherent collection of activities that have as their goal the production of valuable knowledge. In this chapter I examine claims regarding the unique status of psychology as a science. I argue that there are no special features of psychology which clearly demarcate it from other forms of scientific inquiry. This point is illustrated in terms of the wide overlap between psychology and other sciences such as biology, anthropology and sociology. As such, psychology can only be conceived as a quasi-autonomous discipline; however, one that can neither be eliminated by or reduced to other scientific disciplines. Explanations in psychology, I will further argue, are best represented over a distal-proximate dimension. Ideally, explanations over different temporal scales should be developed in conjunction with one another to provide coherent causal pictures. This idea is linked to the two notions of explanation presented in the previous chapter. That is, explanations should be sought in psychology which provide a means of unifying diverse phenomena as well as elucidating the specific causal mechanisms which bring about phenomena. Finally, although psychology is reasonably characterized as conceptually fragmented, I will argue that where possible, psychologists should be pursuing globally integrated systems of theory which provide coherent explanations of psychological phenomena at different levels of analysis and over varying temporal frames.

Is psychology a science?

Questions about the disciplinary status of psychology have come from a variety of sources. Philosophers of science, such as Kuhn, Lakatos, and Popper have all, either explicitly or implicitly, questioned whether psychology deserves to be considered a science in the same sense that either physics or biology is. From within psychology itself there have been many claims (especially from humanist and hermeneutic perspectives) that either psychology should not be considered a science at all, or if so, a radically *different* kind of science.

The main concern of Popper (1959, 1963) with the scientific status of psychology is with respect to its supposed lack of falsifiability. This criticism, although primarily directed against psychoanalytic theory, has also been aimed more generally at psychology as a whole. According to Popper, theories in psychology can always be rescued by illicit *ad hoc* moves. If theories cannot specify the conditions in which they may be refuted, then from Popper's perspective, they necessarily fall outside of the purview of science.

This criticism of psychology as a science is wrong-headed in two important ways. Firstly, theories in psychology, even in psychoanalytic psychology, (Grunbaum, 1986) *are* falsifiable. Falsifications may lead to modifications in the theory in question, but those modifications are then subject to further tests. Secondly, falsifiability as a demarcation criterion, is an inappropriate way to demarcate science from non-science. Importantly, as Lakatos (1978) has made clear, theories are never tested in isolation but are always conjoined with other theories, auxiliary hypotheses, and observational techniques and equipment. If empirical findings fail to support the theory, the scientist can always blame one of the auxiliary hypotheses or some aspect of the experimental set-up. Theories are always under-determined by the data that they purport to explain. Typically speaking, scientists will be working within the framework of a global research programme, where disconfirmations rather than spelling doom for the program are simply viewed as anomalies that await explanation. More generally speaking, it is unclear that there is any criterion or set of criteria which could be said to adequately demarcate science form non-science. Laudan (1983), for example, believes that science is a far too heterogeneous collection of activities to allow for any demarcating criteria. Certainly it is implausible to suggest that there will be necessary and sufficient conditions for what is to count as science, although it may be possible to take a prototype approach to the issue of demarcation, or to characterize science in some more abstract sense as a certain kind of relationship between organisms and the world (e.g., see Dunbar, 1995).

The lack of global theories in psychology have prompted both Kuhn (1962) and Lakatos (1978) to question the disciplinary status of psychology. The lack of any unifying paradigm for Kuhn is suggestive of pre-science rather than science, where various loosely articulated schools of thought vie for adherents. The lack of any theoretical unity is also considered a problem by Lakatos, who in addition has been especially scathing regarding the lack of methodological sophistication often displayed by psychologists.

I think that it is undoubtedly true that psychology lacks any kind of unifying theoretical framework, although it does have its share of genuinely global research programmes. However, this in itself is not an adequate demarcation criterion, nor does it necessarily indicate that psychology is a very different kind of science than say physics or biology. This is especially true if we consider that all theories are more or less global in nature (Hooker, 1975) and that even in psychology limited unification has been attained. The methodological naiveté of many psychologists is certainly an area of concern, as noted by psychologists themselves (Meehl, 1967, 1978; Lykken, 1991). However, this is surely a consequence of bad science or the adoption of an inappropriate philosophy of science, rather than a problem inherent to psychology *per se*.

The disciplinary status of psychology, as well as occupying the thoughts of various philosophers, has also been a perennial concern for psychologists themselves. Ever since psychology split from its institutional and intellectual affiliations with philosophy there have been numerous attempts to characterize just what the nature of psychology is. It would be fair to say, that despite a considerable degree of intellectual labour directed towards this task, there has emerged no consensus of opinion about whether or not psychology is, or should be, considered a science. As Giorgi (1992, p. 46) notes, "Throughout its history, psychology has been described as a natural science, a human science, two or more sciences, as intrinsically non-scientific – and other things too numerous to mention."

One of the main reasons for this divergence of opinion regarding the status of psychology is the extreme diversity of psychology's subject matter and forms of inquiry which have been at different times labeled as psychological. This 'intellectual zoo' (Miller, 1992, p. 40) provides a means of characterizing psychology as scientific or non-scientific depending on just which species of psychology one chooses to consider. Two important divisions that emerge when considering the scientific status of psychology are the ones drawn between academic and professional psychology and between psychology as a science and psychology as an art.

The schism between academic and professional psychology has been highlighted recently by Corballis (1990), who argues for a division, institutionally at least, on this basis. Whether or not professional psychology might be considered a science remains an open question here, although Corballis notes the gradual decline in the scientist-practitioner model and the increasing distance that is being established between professional and academic psychologists.

The most frequently aired source of disagreement over the status of psychology is whether or not the subject matter that psychologists investigate lends itself to a form of scientific inquiry or whether the domain of psychology is more conducive to the sort of inquiry engaged by those in the humanities. That is, there is a division drawn between psychology as a human science and psychology as a natural science. There is a long tradition, epitomised in humanist and hermeneutic approaches, which conceptualises psychological investigation as a primarily *interpretative* enterprise. Those of post-modernist persuasion such as Gergen (1985, 1992) have vigorously pursued this view of psychology. Because meaning is central in human life, psychology should be concerned with the way social roles are realised in cultural contexts, and how the agents concerned interpret these roles. Psychology and the other social 'sciences', from this perspective, are more akin to literary interpretation than the kind of inquiry pursued in the natural sciences (Gergen, 1985, 1992).

One way of resolving this conflict between the science and the art of psychology would be to admit that some psychological phenomena fall clearly within the scope of natural scientific methods, while other psychological phenomena lie outside the purview of what might be considered science. William Wundt, remembered mainly as the founding father of experimental psychology, was one prominent proponent of this kind of division (Toulmin and Leary, 1992). For Wundt, only certain aspects of psychology, such as basic sensory and perceptual processes, were considered to be amenable to empirical investigation using the methods of the natural sciences. Other areas of psychology such as language, custom, social structure, were viewed as more appropriately investigated using other methods such as those employed by anthropologists. Psychology on the view presented above would cleave into two separate disciplines: one, a science concerned with basic physiological, perceptual and cognitive processes, and the other, a humanity engaged in the elucidation of personal and social meaning.

If our conception of science is modeled on Newtonian physics (as is not infrequently the case in the history of psychology), and formalised in the empiricist (specifically logical positivist) tradition, then it is clear that many if not most branches of psychology fall outside the scope of scientific inquiry. The beliefs, desires and other intentional attitudes of humans are widely (e.g., Manicas & Secord, 1983, Secord, 1990, Robinson, 1985, Margolis, 1990), although not universally (e.g., P. M, Churchland, 1989), considered to be an ineliminable part of many psychological explanations. However, it is extremely unlikely that we will discover any laws of human action in the way that we have laws of say gravitation (Manicas & Secord, 1983). Human action is "spatially, temporally, and socially situated" (Secord, 1990, p. 79), therefore explanation as subsumption under the covering law model will be inappropriate for psychology. Furthermore, because of the variability of psychological phenomena, psychology will never achieve any measure of predictive power. It is highly unlikely that we will be able to say, with anything like deductive certainty, that certain states of affairs will be obtained regarding human thought or action given certain initial conditions and any putative psychological laws.

The open nature of psychological systems also poses problems for the role of experiment in psychology (Margolis, 1990). The closure demanded by experimental procedure radically alters the behaviour of the subject and hence the object of inquiry. This problem is exacerbated by the double hermeneutic: people are both the subject of inquiry and the inquiring subjects. For many psychological phenomena, especially of a social nature, the role of experiment in psychology is likely to be strictly limited and alternative methods of investigation will have to be employed.[1] As Manicas and Secord (1983, p. 410) express it: "If our aim is to explain behaviour as it occurs in ordinary life there is no escaping the ordinary description of behaviour and experience."

Clearly if psychology is to be considered a science it will be a very different one from that of experimental physics and will not conform to a model of science informed by most versions of empiricism. Many of the conceptual problems regarding the disciplinary status of psychology might, however, be resolved on alternative philosophies of science. Giorgi (1992, p. 47) presents a clear statement of this view:

> Were a theory of science which is compatible with the diversity and complexity of human reality to be attained, the problems of psychology's unity, of the scientist professional dichotomy, and of the meaning of psychological science could, in principle, be resolved.

[1] Although Greenwood (1982) has argued that causal inquiry based upon closed experiments is possible, even in social psychology. Greenwood suggests that although there are many barriers in practice to developing a viable experimental social psychology there is nothing in principle to suggest that experiments cannot make an important contribution to an understanding of social behaviour.

In the first chapter of this thesis I have articulated a realist approach to science which I think can do adequate justice to the nature of psychological inquiry and which can resolve, or at least provide a means of resolving, some of the perennial problems regarding the disciplinary status of psychology. It will become clear that although psychology is not a science like physics, it is nonetheless a legitimate form of intellectual inquiry, more in line with the sciences of biology or geology in terms of the kinds of explanations that it seeks. Moreover, I will argue that although psychology's subject matter is in some respects unique, nonetheless it can be coherently understood as part of a larger enterprise, continuous with that of both the natural and social sciences. In developing this view of psychology it will be useful first to consider in more detail the domain of psychological inquiry.

The domain of psychology

One of the reasons for the widely differing attitudes regarding the scientific character of psychology is the sheer diversity of psychology's subject matter. Psychologists study a tremendous range of different kinds of phenomena which exist at different levels of structural organisation and which can be understood at different levels of analysis.

The structural organisation of nature, as traditionally understood in science, can be conceived as a series of part-whole relations. Cells are parts of organs, which are parts of organisms, which are parts of ecosystems and so on. The subject matter of psychology embraces a wide cross-section of this physical stratification, from the chemical properties of nerve cells through to the ecological relations of humans and society.

Whereas levels of organisation are concerned with the part-whole relations of the physical world, levels of analysis refer to a conceptual distinction in terms of the kinds of questions that can be asked of the world. Although our best scientific picture of nature is a thorough going physicalist one, there are also various emergent properties of this physical world which are best understood at different levels of conceptual analysis. Much of psychology is concerned with a portion of these emergent properties: mental phenomena, intentional behaviour, and the social relations between individuals. Other disciplines such as anthropology and sociology are concerned with emergent properties at more inclusive levels such as social structure and cultural traditions.

One popular approach to explaining psychological phenomena is to draw the distinction between three different levels of analysis: ecological, computational and implementational (e.g., Marr, 1982; Stereleny, 1990). The ecological level[2] specifies the functional attributes of the system; the computational level explains how the system operates algorithmically while the implementational level explains how the computational level is realised in a physical system. It is important to note that all these levels should be considered widely, that is, in reference to the appropriate environments (Stercleny, 1990).[3]

I will adopt a similar division in my conceptual partitioning of psychological phenomena, although with some clarifications. I will conceive psychology as being concerned with four levels of analysis: functional, physiological, psychological, and social. The functional level, as in the ecological level, specifies the purpose of the system in question. It is here that we ask the question: 'What is it for'? Where *for* is a short hand for 'what are the adaptive advantages of the system in question in the environment in which it was selected for?' Functional questions can be asked about each of the three other levels of analysis. We can licitly inquire about the function of a brain system, a cognitive system, or certain kinds of social arrangement. This level is a crucial one, for questions of function help us to demarcate real phenomena from noise. Moreover, knowing what something was *designed* to do can provide us with important information about *how* the system is likely to carry out these functions.

[2] I adopt Stereleny's terminology in my presentation here. Marr's original formulation, somewhat confusingly, labels the ecological level as computational and the computational level as algorithmic.
[3] It should be clear here that although there is some overlap between levels of analysis and levels of organisation, the mapping is only partial. Psychological phenomena, for example, have no obvious place in our part-whole physical ordering of the world. We cannot say that memory is part of brains or that neurons are parts of memory, even though memories no doubt involve the action of many neurons, which are located in the brain. Part-whole orderings, however, do operate at the psychological level of analysis, although the relations are psychological ones.

The physiological level of analysis is concerned with the full range of physical processes. In psychology these processes mainly include brain functions at various levels of organisation, from neurons through to hemispheric functioning. At the psychological level of analysis important phenomena include intention, memory, imagery, consciousness, and the full range of mental states typically studied by cognitive psychologists. Levels of organisation also emerge at this level of analysis with psychological systems being embedded in larger systems in our overall cognitive architecture (e.g., Lycan, 1987; Dennett, 1978). The psychological level is seen to be supervenient on the physiological level. That is, although for each psychological state there is a physical state, the psychological level displays emergent properties not specifiable at the physical level. It is at this level of analysis that psychology claims autonomy from biology on the one hand and sociology on the other (Margolis, 1990). However, this autonomy can only be considered a relative one, because a full understanding of psychological states requires an understanding of their functional properties, their physical instantiation, and their environmental (often social) referents. Psychological phenomena, therefore, are biosociologically constrained (Bunge, 1990). Moreover, psychological states are important parts of sociological explanations and are themselves part of the subject matter of biology (e.g., cognitive ethology; Ristau, 1991).

The social level of analysis is also an important one for many psychological phenomena. For as Manicas (1987) and others (Margolis, 1990; Secord, 1990) point out, much of the subject matter of psychology is concerned with the emergent properties of persons as they appear in social environments.

> Persons are surely minded, but they are best conceived as culturally emergent in exactly the sense that they have capacities (properties, causal powers) - for example, linguistic abilities - predicable of them only by virtue of the causal outcomes of the development of their biological complexity in a social environment. (Manicas, 1987 p. 296)

Just as the psychological level can be said to be realised by the physiological one, so too are social phenomena realised in psychological processes. That is, social phenomena such as status ranking or group decision-making are dependent on, but not reducible to, the thoughts and actions of individuals.

It is important to note that each level of analysis provides important constraints on theorising at other levels. Therefore, our best theories of psychological phenomena should be coherent with our best physical and social theories, and should be functionally realisable. Furthermore, these levels of analysis, although broadly demarcating the range of phenomena studied by psychologists, are better conceived as continuous rather than discrete in nature. This is reflected in the various areas of inquiry which occupy spaces at the interstices of these levels such as cognitive neuroscience and social cognition. Moreover, many areas of psychology, such as emotion and development, transcend these boundaries and study phenomena across different levels of analysis.

A brief reflection on the levels of organisation in nature that psychologists are interested in reveals that there is a considerable overlap in what psychologists study and what is investigated by biologists, sociologists, and anthropologists. Biologists are clearly concerned with the physiological underpinnings of brain and behaviour as well as the evolution of mind and social behaviour. Anthropologists investigate the psychological, social, and cultural aspects of human nature, as it is manifest across different environments. And sociologists are interested in the rich network of social relations and institutional features that play a role in influencing human behavour. Psychologists also study all these domains of interest.

Psychology therefore, as Bunge (1990) suggests, can be considered as a *quasi-autonomous* discipline, one with important connections to both biology and sociology. It follows that one way progress can be achieved in psychology is through a coherent integration of theories at different levels of analysis. The development of interdisciplinary studies such as cognitive science and sociobiology can be seen as attempts to provide this conceptual integration. Ultimately, a complete understanding of the range of psychological phenomena will be obtained by providing detailed accounts of bio-psycho-social *systems* rather than discrete and localised analyses of a narrow spectrum of the organisational hierarchy.

Questions regarding psychology's status as a science typically reflect a concern that the psychological and social levels of analysis are not explicable given the methods of natural science. A commitment to producing a coherent understanding of psychological phenomena renders this position somewhat problematic. It is conceptually messy, not to say counter-productive, to demarcate the science of psychology from the art of psychology on the basis of some reasonably arbitrary cut in the hierarchy of organization in nature or in terms of specific levels of analysis which are mutually interdependent rather than autonomous. That is not to say that phenomena at different levels of analysis or different levels of organisation all need to be investigated using identical methods. As I will argue below, the richer understanding of science afforded by a naturalistic-realistic perspective offers a way of providing a conceptual integration of the way the diversity of psychological phenomena should be understood.

A naturalist-realist perspective on psychological science

Anti-naturalist approaches to psychology maintain that psychology should be considered as radically discontinuous with the natural sciences and should develop its own methods and means of evaluation for investigation of its subject matter (e.g., Margolis, 1990). The push to establish psychology as a unique science independent from the natural sciences is interesting in the light of similar claims (for similar reasons), made by biologists (e.g., Mayr, 1982) for the autonomy of their own discipline. For example, Mayr (1982) points out, as psychologist do for their own discipline, that biology does not typically involve subsumption under universal laws[4]. Indeed, outside of some areas of physics,

[4] There *are* of course laws in both biology and psychology, but they tend to be more limited in application than those in physics.

explanations in science do not typically involve the invocation of laws at all. Furthermore, biology, geology and other sciences, like psychology are not strongly predictive in nature. The problem in establishing a science of psychology, therefore, is not one of precision. Many sciences subsume explanations under probabilistic rather than universal laws. This is certainly true of evolutionary biology, as well as many of the geological sciences, notoriously of course, meteorology. Indeed, the activity of any dynamic system is going to be difficult to predict with any degree of accuracy. Explanation broadly speaking, therefore, as Kitcher (1981) argues is more fruitfully understood as unification rather than subsumption under covering laws. Scientists, in many diverse areas of inquiry, seek to unify phenomena by instantiating a small number of basic argument patterns or types.

Explanation in biology, as in psychology, also has an essentially narrative structure. Because biological systems are characterised by variability, randomness, and complexity, accounting for individual action involves the invocation of historical narratives (which are individually unique) over various levels of temporal resolution (Mayr, 1982). Moreover, both biological and psychological phenomena are strongly context dependent. An investigation of individual structures can not proceed without due attention to the appropriate context, be it physical, ecological, social or cultural. Experiments in biology therefore, as in psychology, will have to be sensitive to this context dependency. As such observation and comparison become legitimate scientific strategies for coming to understand the nature of the phenomena in the domain in question (Mayr, 1982).

It would seem then, on the basis of this preliminary analysis, that the bifurcation of psychology from the natural sciences cannot be sustained. Neither psychology's subject matter, explanatory style, or methodology can justify its separation from the natural sciences. Psychologists are right to reject physics as the model science from which to compare their discipline. However, few sciences live up to the inappropriate demands to be a 'science like physics', and psychology can be likened in some important respects to the natural sciences of biology and geology broadly conceived.

The foregoing suggests that the important issues regarding psychology's status as a science pivots not so much on the nature of psychology but on the way science itself has been and continues to be conceived. Certainly as long as psychologists remain adherents to the 'cult' of empiricism (Toulmin & Leary, 1992) with its commitment to hypothetico-deductive method, the covering law model of explanation and an instrumentalist construal of theory, much of psychology will lie outside of the domain of science. This impoverished view of science, however, as has been widely, but far from universally recognised, cannot be sustained.

One alternative would to be pursue one of the various social-constructivist approaches to science. This alternative is actively developed by a variety of psychologists in various different guises (see for example, Gergen, 1985, 1992; Packer, 1985; Hare-Mustin & Maracek, 1988). For reasons that I have elaborated upon in detail in chapter one, and which are aptly summarised by Greenwood (1987, 1992), post-modern perspectives fail to adequately portray the practice of scientists, the history of scientific thought, or the nature of reality itself. There is no reason to believe that the portion of the world investigated by psychology warrants a radically different philosophy of science, than that which best characterises the rest of scientific inquiry.

Human belief, desire, and other intentional attitudes are certainly *different* from the phenomena studied by physicists or bio-chemists. However, there is no reason to believe that these phenomena are not also real and explanatory in the same sense that say quarks or genes are. From a realist perspective, psychological and social states are real properties of the world, which have real effects. That is, they can be invoked in the development of causal explanations of the manifest facts that psychologists study (Greenwood, 1988, 1992; Manicas and Secord, 1983; Secord, 1990; Bunge, 1996). Psychologists then, like other scientists, should favour the construction of deep or postulational theories, which go beyond the data to invoke the operation of hidden causal mechanisms. That the mechanisms in question are psychological or social rather than physical in nature, should be no barrier to their development and use.

It is worth emphasising at this juncture that explanation should not be conceived of as subsumption under generalised laws. Psychologists are surely right in their general, although not universal agreement, that there will be few if any true laws in psychology. However, this is no warrant to reject the hope of developing adequate explanations in psychology completely. As I have detailed in chapter one, explanation is more fruitfully understood in terms of unification and with respect to the elucidation of causal-mechanistic processes. It is by invoking a small number of argument patterns to explain a range of phenomena and by detailing the mechanistic processes that give rise to the facts that we wish to explain, that our understanding to the world is promoted.

I contend that this approach to explanation can be just as usefully applied to psychology as it can to other areas of intellectual inquiry. All humans are certainly unique and hence there are unlikely to be any laws of human action (Lykken, 1991; Robinson, 1985). To explain the individuality of humans we must invoke a myriad of historical, social, personal, genetic, physiological and cultural factors. This however, is true of *any* biological system and suggests that psychological explanations must advert to a multiplicity of specific causal processes of various kinds: physiological, psychological, social and so forth. This is just the kind of multi-dimensional explanation that is sought by biologists attempting to explain specific patterns of animal behaviour, or by meteorologists in explaining specific patterns of weather. Psychology, like biology or meteorology is, therefore, not a *predictive* science, or at best, only a *weakly* predictive one[5]; however, there is no reason to suggest that it is not an *explanatory* one.

The importance of meaning and context in human action does not invalidate this comparison of psychology with the other sciences in terms of the kinds of explanations that it should be seeking. It is certainly true that the intention of human agents plays an important role in the explanations of their behaviour. Moreover, behavior can often only be understood through reference to its embeddedness in particular social contexts. However, it is unclear how these facts of human life prompt a rejection of explanation in

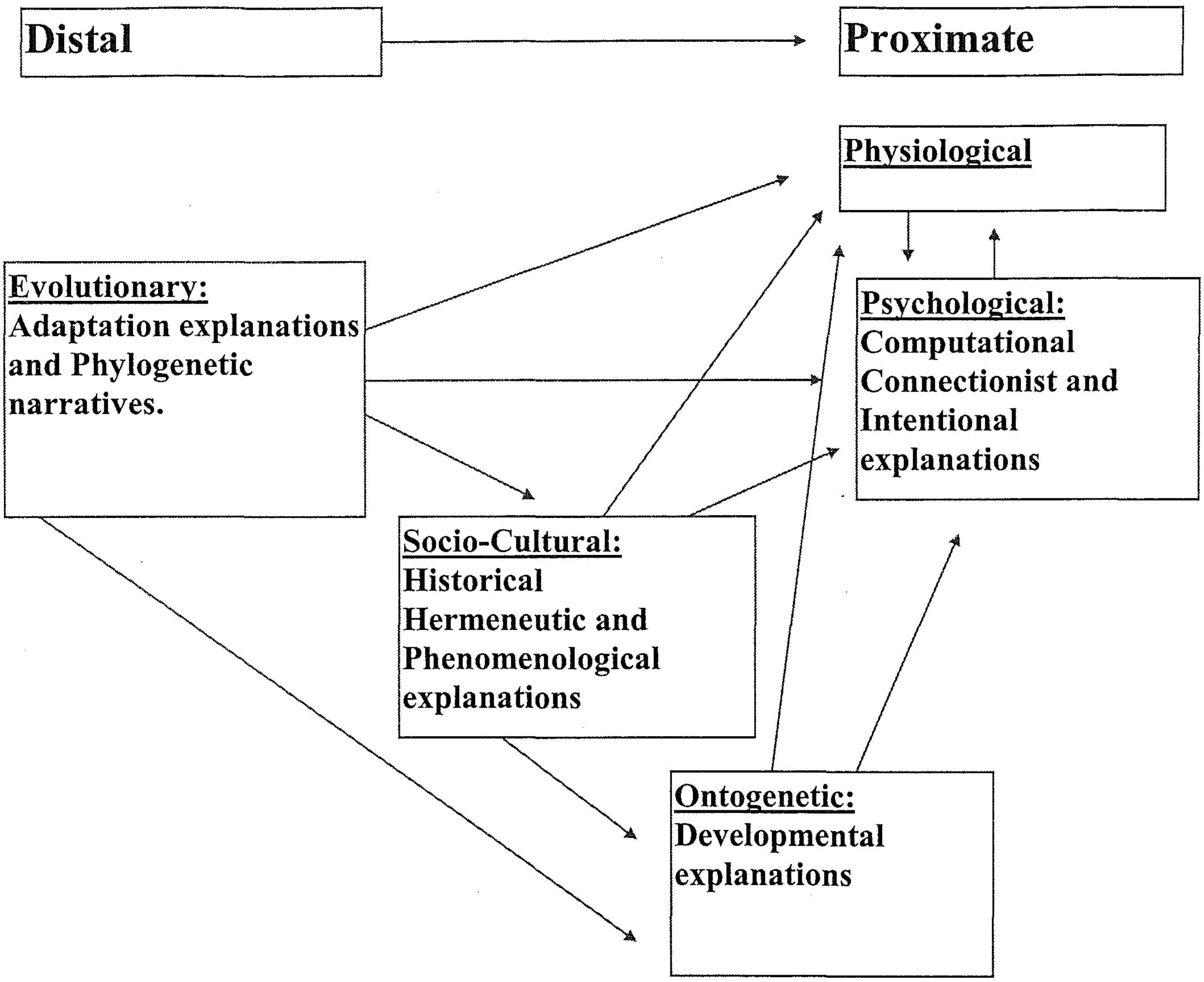
[5] Given enough background information about an individual we can do considerably better than chance in predicting their behaviour given certain initial conditions. We are at least as reliable in this task as the average weather forecaster is in predicting specific patterns of weather.

favour of interpretation as a means of furthering our understanding of psychological phenomena. Providing rich, detailed, and sympathetic accounts of human behavior is an important aspect of psychology. However, I see this process fundamentally as a way of *elucidating phenomena* which it is the job of psychological theory to explain, rather than an end point in itself. That is, in providing an understanding of what it means to engage in certain patterns of social discourse we have uncovered some interesting empirical regularity about the (human) world which it is the job of psychological theory to explain.

To advance our understanding of human action we need to go beyond description and interpretation to explanatory accounts of why certain instances of behaviour occur. Ideally these explanatory accounts will draw on both general features of humans under certain environmental contexts (i.e. unifying explanations) as well as specific details of local processes (i.e. causal-mechanistic explanations). That is, we want to explain both the regularities and invariances of human thought and action as well as accounting for specific individualistic instances of these phenomena. In science generally these two kinds of explanation are usually intimately connected. Ideally we should be able to invoke general argument patterns which provide explanations for a wide range of phenomena, while at the same time detailing the specific mechanisms which realise these general processes.

Explanations in psychology can also be usefully conceptualised over a distal-proximate dimension. Figure three illustrates the range of explanations considered over different temporal frames. Explanations drawn at different temporal levels are likely to be intimately related to one another in a variety of ways. For example, we can invoke the history of an individual's personal development as part of the explanation for the kinds of beliefs and desires that they hold. Moreover, this individual history may be explicable given certain specific cultural antecedents. The specific details of the culture may in turn be explained by invoking evolutionary considerations of function that may themselves be illuminated by reference to certain psychological processes. To use a crude example, Bob's desire for pork and belief in the acceptability of porcine consumption may be explained by an upbringing, rich in crackling and apple sauce, explicable given prevalent

Figure three Levels of Explanation in Psychology



cultural beliefs in contemporary western (but not Muslim) cultures. The institutionalisation of livestock farming more generally may reflect an evolutionary history where humans have undergone selection for mechanisms which promote the consumption of animal flesh and the knowledge and control of livestock.

The causal web here is clearly complex, and this is a simple example. However, like all other sciences, psychology should be aiming for complete explanations of the phenomena in its domain. Ideally we should be able to sketch out the physical, psychological, social, cultural, developmental, and functional processes which give rise to the phenomena of interest. Clearly it is a gross understatement to say that this is no easy task. It is likely that for any domain of interest in psychology there are multiple, interactive, causal processes in operation. It follows that one way progress in psychology might be achieved is through the development of increasingly richer connections between theories constructed at different levels of analysis and over different temporal frames. This necessitates not only increasing levels of communication between psychologists themselves, but also more elaborate connections between psychology and other sciences as well as between psychology and philosophy.

Just which part of the ideal explanatory text that we will wish to illuminate will be, as noted in the previous chapter, determined in part by pragmatic concerns. The kind of explanation seeking why questions formulated by professional psychologists are likely to be somewhat different from those motivated by less practical concerns. However, progress in both clinical and academic psychology is only likely to be achieved to the extent that the sorts of explanations that are developed accurately capture the nature of real processes in the world.

If our goal is the production of valuable truth, then we must pay due attention to the rich and complex connections that exist between academic and professional psychology as well as those between psychology, public policy and society. It is my contention that these aims are best achieved through the development of global research programmes. In the following sections I discuss the disunified nature of psychology as a science and indicate ways that unification might be achieved in a manner commensurate with the goals of science.

Systems of psychological explanation

Psychology as a disunified science

Psychology, if portrayed as a science at all, is often depicted as a science in disarray (e.g., Koch, 1981; Staats, 1989; Royce; 1985; Bevan, 1991; Giorgi, 1992). For example, Staats (1989, p. 149) suggests that, ". . . fields of psychology have developed as separate entities, with little or no planning with respect to their relationships. Research areas grow in isolation without ever being called on to relate themselves to the rest of psychology." The disunity of psychology is manifest in various ways: institutionally, methodologically, and theoretically. At the theoretical level, for example, psychology is characterised by a proliferation of small-scale mini-theories (often little more than descriptions of empirical regularities), which are employed to explain a limited array of psychological phenomena. The growth of these local theories, moreover, is rarely pruned through judicious comparison with alternative theories and is not, typically speaking, integrated with other theories in different branches of psychology.

The apparent disunity of psychology can perhaps be explained by psychology's relative youth as a science and the intrinsic complexity of its subject matter (Royce, 1985; Staats, 1989). Staats (1989) argues that there is a progression in the natural sciences from periods characterized by disunity and a proliferation of local theories, towards more integrated unified disciplines. In some senses this echoes Kuhn's (1970) analysis of science which portrays disciplines as emerging from a pre-paradigmatic state, characterized by a diversity of competing schools, to a relative unity of research and theory within the boundaries of a given paradigm. However, as Kuhn would have it, science over time enters cycles of unity and disunity during periods of normal and revolutionary science.

The idea of science becoming increasingly unified over time is perhaps questioned by the often prolific growth of sub-disciplines in science. As new phenomena become accessible to scientific inquiry through developments in theory, methodology, and research techniques, the depth and breadth of disciplines increase. In biology, for example, the last fifty years or so has seen a plethora of new sub-disciplines, such as molecular biology, biochemistry, behavioral genetics and paleo-biology. However, in one important sense these sub-disciplines *are* unified in that they all, ultimately, appeal to the Neo-Darwinian theory of evolution. For psychology however, there is no such over-arching theoretical perspective which provides a similar degree of conceptual unification. The search for some kind of global theory in psychology is seen therefore by many to be a virtue (Staats, 1989; Marx & Cronan-Hillax, 1987). Marx and Cronan-Hillax (1987), for example, argue that psychology will become a mature science when it moves from its present pre-paradigmatic phase into paradigmatic science. The advance of science, it is argued here, is best served by theoretical monism: the construction of a single grand theory which serves to explain and organise the diversity of phenomena in a given domain.

By comparison, the lack of unification in psychology has been argued by some to be a virtue, or at least an unavoidable consequence of, psychology's unique subject matter (Koch, 1981; Dixon, 1983; Gardner, 1995). Both Koch (1981) and Gardner (1995) argue that psychology will never become a coherent unified discipline because the various sub-fields of psychology are so fundamentally different from each other. Psychology therefore, should be best conceived as a family of related sciences rather than a single unified enterprise. Dixon (1983) suggests that " psychological phenomena are sufficiently dynamic, interdependent, and multi-dimensional as to require multiple theoretical renderings and multiple modes of inquiry." Similarly, Little (1991) argues that the search for a unifying theory in psychology is mistaken because social and psychological phenomena are too diverse, complex, and open-ended to make any coherent over-arching theory viable. The best strategy for psychology therefore, it is argued, is to pursue a vigorous theoretical pluralism. That is, psychology should construct a diversity of theory commensurate with the highly heterogeneous nature of its subject matter.

Psychology's subject matter, as elaborated upon earlier, includes consciousness, intentionality, the nature of self, as well as perceptual, cognitive, and neural processes. However, there is nothing in principle that precludes the possibility of an approach that makes explicable all of these areas of inquiry within a single theoretical framework, while specifying how phenomena at different levels relate to each other in meaningful ways. I would argue that other areas of academic inquiry, such as biology, have a similarly diverse subject matter, but do manage to attain some degree of theoretical unity.

Theoretical pluralism is surely necessary for the growth of science. Progress cannot be achieved unless there is an active development and comparison of alternative theoretical perspectives. As Royce (1985) notes however, theoretical pluralism manifests itself in different ways in different kinds of science; *sequentially* in mature sciences and *simultaneously* in immature ones. The initial push for explanation of phenomena in an immature science will see the development of a legion of different, highly localised theories. As Royce (1985) suggests, this is the only way the immature science can attempt to cover the full scope of its subject matter. Over time, however, there should be an attempt to produce theories of ever increasing explanatory coherence. Importantly, theories should be developed which are capable of explaining a wide range of diverse phenomena using a few basic argument patterns. Royce (1985, p.11) suggests that "The recommended pragmatic action [for psychology] is to promote the proliferation of potentially viable theories and simultaneously to develop a small number of theories that show the greatest potential for eventually becoming conceptually powerful."

There are two primary means whereby unification might be achieved. One is through the reduction of phenomena and the other is though the development of global research programmes. I shall consider these two options in turn and argue that psychology, like other sciences, is best served through the construction of GRP's which serve to unify the domains of interest. However, this view retains the idea that progress is achieved through the development of multiple theories of various degrees of globalness. Just what kind of theories will be preferred in psychology will be determined on the basis of the multiple criteria for theory appraisal outlined in chapter one, and will be commensurate with the goal of producing valuable knowledge.

Reductionism in psychology

The notion of the unity of science has traditionally meant, as Trout (1991, p. 338) notes that "all events (properties, states, processes, etc.) that are implicated in the laws of any special science fall under the laws of physics." For psychology reduction has been construed in various ways. For example: from social to individual processes; from mind to brain and from mind to behaviour. Perhaps the most important and historically prevalent kind of reduction is the idea that psychology can be reduced to neuroscience.

The strong portrayal of the identity thesis of the mind suggests that psychological types can be reduced to physical or neural types, so that (with the appropriate bridge laws) we could literally translate psychological phenomena into physical phenomena, with no loss of information or understanding. Given the multiple realisability of mental states[6], however, this strong thesis was relaxed so that while each psychological state is conceived as physically *instantiated,* there is no lawful correspondence between psychological kinds and physical kinds (Burge, 1990). The autonomy of psychology from neuroscience has been defended by the majority of theorists in the philosophy of mind (e.g. Fodor, 1991b; Stereleny, 1990; Dennett, 1981; Lycan, 1987; Burge, 1990). It is argued that there are important generalizations at the psychological or functional levels that are not captured by theories in neuroscience. Neurophysiological theories of the brain simply do not explain, by themselves, the wide range of cognitive phenomena. However, psychological theories, of course, can never be entirely free of the considerations drawn from the neurosciences. For psychological phenomena there seems to be a partial dependence on, but not a reduction to, neural states.

[6] This is the idea that similar mental states can be realised by different neural states in different people, animals and (potentially) Martians, and in the same person at different times.

An alternative portrayal of psychology's relation to neuroscience is to suggest that psychological phenomena can be entirely *eliminated* by neuroscience (Rorty, 1971; P. M. Churchland, 1981, 1985). Paul Churchland (1981, 1985) argues that folk psychology is a radically false theory and that its conceptual machinery should be dismantled and discarded in favour of the more powerful theories of a completed neuroscience. Instead of 'feeling pain' we should talk about the 'firing of C fibres' (Rorty, 1971), and instead of 'having a belief that there is a wombat over there' we should say[7] 'there is a spiking frequency of 350 HZ in neural pathway X at the sixth level of the occipital cortex' (Churchland, 1985).

The huge advances in neuroscience and Churchland's eloquence make such a proposition less absurd than it might initially seem. The arguments against eliminativism, however, seem as strong as those against psychological reduction. Jackson (1982, 1986) argues that there is knowledge above and beyond physical knowledge, such as the way things introspectively feel, which is an important part of psychology. It is also not clear how eliminativism escapes the argument of multiple realisability that has been leveled at the identity thesis. My belief about wombats has a different neural instantiation, no doubt, than does yours (or that of a wombat for that matter); so even if we were to be hooked up to one another by splicing the nerve ends in our brains together (as Churchland suggests in one of his scenarios) we would not be able to understand one another. In an important paper McCauley (1986) argues that eliminativism fails because theories in neuroscience and psychology operate at different levels of analysis.

> The mistake *all* versions of eliminative materialism make is to draw their eliminative conclusions about the inter level relationship between psychology and neuroscience on the basis of analysis of intra levels contexts . . . *the history of science reveals no precedent for theory replacement or elimination in interlevel* contexts.
>
> (McCauley, 1986 p.197).

[7] Although in Churchland's scheme (1985), we do not have to *say* anything. With practice we should be able to directly introspect brain processes.

Skinner (1974) of course also pursued a form of elimitavism in psychology, in his programme of radical behaviorism. For Skinner the psychological level could be eliminated in favour of descriptions of patterns of behaviour in relation to the contingencies of the environment. In general, the sorts of criticisms directed against the elimitivism of Churchland are likely to hold also for Skinner's eliminative programme.

Attempts at unification in psychology through reduction or elimination of psychological phenomena have proven untenable. An alternative approach to achieving unification in psychology is through the development of global research programmes that promise to make coherent the diverse collection of phenomena studied by psychologists.

Global research programmes in psychology

The disunified state of psychology can be attributed, in part, to a lack of a single, coherent, overarching theoretical framework. Unlike physics, biology, or geology, psychology does not possess a GRP or GRP's of sufficient coherence to unify the diversity of its subject matter. However, text-book presentations of psychology (e.g., Atkinson, Atkinson, Smith, Bem, & Nolen-Hoeksema, 1996) often make distinctions between various different *approaches* or *perspectives* in psychology. For example, Atkinson et al. present what they view as five different approaches in psychology: behavioural, biological, phenomenological, cognitive, and psychoanalytic. This collection of different approaches, however, is mix of *frameworks*, *domains* and *research programmes*. GRP's are those large-scale collections of theories, methodologies, and so forth elaborated upon in chapter one. Frameworks are more diffusely organised ways of approaching a given subject matter characterised by particular methods and often limited to specific domains. Domains of inquiry represent different portions of the world at different levels of analysis or structural organisation. Finally, theories refer to those specific models of the world, which are employed to explain patterns of data.

From this perspective, cognitive and biological psychology are better characterised as domains of inquiry, which focus on psychological phenomena at different levels of analysis. Although there are specific cognitive and biological theories of psychological phenomena they are not typically organised in any unified way.[8] In the sense that cognitivism has a specific collection of experimental methods and explanatory styles, it might also be termed an approach or a framework from which to organise inquiry. The labeling of behavioral and psychoanalytic perspectives as approaches, while in some sense accurate, obscures the fact that there are also coherent GRPs roughly co-extensive with these labels. Both radical behaviorism and Freudian psychoanalytic theory are organized in such a manner as to quality as GRPs in much the same sense as quantum physics or Darwinian theory are (although they have achieved somewhat less success). Radical behaviorism, for example, as formulated by Skinner (1938, 1974), possess a series of core assumptions including certain laws, a variety of auxiliary theories, specific methodologies, technologies, and a proposed programme of social action (see Skinner's utopian novel *Walden II*). Radical behaviorism, therefore, offers a truly coherent *world view*.

The discussion of science in chapter one suggests that one way that progress in achieved is through the development of theories with ever increasing explanatory coherence. In the natural sciences the development and comparison of GRP's have typically achieved this. GRP's advance our understanding of a given area of science by providing explanatorily coherent theories, by developing and employing appropriate methodologies and technologies and by specifying satisfactory means of advancing our specific moral aims. The important questions for psychology here, is whether our understanding of psychological phenomena has been, or likely to be, also best served by the development of GRP's.

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[8] Although there are exceptions here. See for example, in the cognitive domain, Anderson (1983) and Newell (1990), and in the biological domain, Edelman (1989)

Progress and conceptual change in psychology

One popular characterization of psychology is that it has progressed through a series of psychological revolutions of a Kuhnian nature. This perspective on the history of psychology suggests that introspectionist approaches to psychology at the beginning of the twentieth century were overthrown in favour of behaviorism which itself was toppled in the cognitive revolution of the 1960's and 1970's.

The application of Kuhn's ideas to psychology, although widespread, have recently been criticized by a number of authors (e.g., Gholson & Barker, 1985; O'Donohue, 1993). It is suggested that the kind of Kuhnian revolutions that have been proposed for psychology simply have not occurred. As Leahey (1991, 1992) argues, the change of focus from introspectionism to behaviourism to cognitivism in psychology, were gradual in nature, and not precipitated by any specific anomalies in the supposedly supplanted approach. Reasons of explanatory coherence, as Thagard (1992) suggests, played only a minor role in this shift in emphasis, which occurred for primarily methodological reasons. It is difficult to characterise the cognitive approach as a *better* alternative than behaviourism although it does offer the possibility of greater explanatory coherence by admitting a wider range of phenomena to be explained. However, it would be fair to say that no one approach in psychology has anything like conceptual hegemony. Indeed, as a recent citation analysis (Friman, Allen, Kerwin, and Larzelere, 1993) suggests, although there have been an increasing number of citations to core journal in cognitive psychology since 1979, there has been no corresponding decrease in citations to core behaviorist journals. Either the behaviorists are taking a long time to die off, or else psychology is still characterised by a plurality of evolving approaches, none of which have ultimate supremacy.

Psychology, is often portrayed by its adherents as a science which is notable for its lack of progress (e.g. Lykken, 1991). Lykken argues that compared to other sciences, psychology has made little progress over the last century or so. As the foregoing analysis of the 'mythical' revolutions (Leahey, 1992) in psychology suggests, there has been no real cumulative progress of theory in psychology, and our understanding of psychological phenomena has not been considerably advanced. Although I think that Lykken is somewhat pessimistic here, it is certainly true that psychology has not progressed this century to the extent that physics, geology, or biology have.

Lykken (1991) highlights a number of reasons for this lack of progress. These reasons can be roughly partitioned into three categories: methodological, theoretical, and institutional. At the methodological level psychologists too often remain committed to an inappropriate and inadequate empiricist approach to scientific method (Meehl, 1967; Lykken, 1991) with an emphasis on hypothetico-deductive method and null hypothesis testing. These methodological woes are abetted by institutional features such as the conceptual divisions in psychology departments, the lack of adequate funding and an economic environment which promotes frenetic experimental activity at the expense of more detailed reflective analysis (Watchel, 1980). Finally, the construction of theory in psychology is pursued in a manner that promotes the proliferation of a plenitude of mini-theories with highly restricted domains. Moreover, there is little attempt to relate these theories to one another in a manner which might promote the construction of more inclusive theories with greater scope and wider application.

Ideally the construction of coherent and well-articulated GRP's in psychology would promote some means of ameliorating these problems. This would be especially true if the programmes were developed in a manner which favored the construction of connections between different GRP's of various degrees of globalness and in a manner which promoted institutional reorganization and an approach to science which paid due attention to the role of both epistemic and non-epistemic values in intellectual inquiry.

Whether or not psychology is likely to best served by the construction of a single unifying GRP remains an open question. The diversity of psychology's subject matter and a century of trying, suggest that this might not be the case. However, that does not preclude the possibility of developing multiple programmes of various degrees of globalness which serve co-operatively to further conceptual, empirical and social progress in psychology.

Summary

I have argued in this chapter, following Bunge (1990), that psychology is best characterised as a quasi-autonomous science, one with important connections to both biology and sociology. Psychology as a science spans a number of different levels of analysis and organisation in nature. Furthermore, explanation in psychology can fruitfully be pursued at both proximate and ultimate levels and in terms of both unification and the elucidation of specific mechanisms.

In its current condition psychology is in a state of conceptual disarray. Psychology is a disunified science. Although theoretical pluralism is to be considered a virtue in science, there is also a need to actively to pursue general frameworks in which the construction of theory can be advanced in a manner commensurate with the multiple explanatory tasks of science. To employ a botanical metaphor: it is better to nurture a range of flowers in the controlled environment of glasshouse, than to toss handfuls of seeds to the wind and hope.

Psychology, like other sciences, should be seeking to develop increasingly unified theoretical accounts of the phenomena that fall in its domain. The most viable way for this to be achieved is through the construction and integration of GRP's which are capable of providing a means to further our understanding to the world and our place in it. From a naturalist perspective, the world is a coherent and unified entity, In the ideal explanatory text of science, the chapter of psychology needs to be fleshed out in a way which does justice to the domain of psychology itself, to the rest of science, and to society.

Chapter three

The evolutionary programme in psychology

The great biologist Theodosius Dobzhansky (1973) once published an article titled "Nothing in biology makes sense except in the light of evolution". Evolutionary theory is our only viable explanatory account for the diversity of biological phenomena on this planet.[1] Of course, as Sober (1993a) notes, many areas of biology such as biochemistry and ecology are not directly concerned with answering evolutionary questions. The day to day activity of a large number of biologists could proceed without explicit reference to evolutionary theory at all. Biochemists elucidate chemical pathways, anatomists lay bare the inner workings of organisms, while ecologists model the dynamics of food webs in ecosystems, without necessarily referring in any way to the theory of natural selection.

However, once the importance of understanding biological phenomena at different temporal and spatial levels is made clear, and evolutionary theory itself is conceptualised in terms of a global research programme, the veracity of Dobzhansky's famous remark becomes more apparent. As I have pointed out in chapter three for psychology, biological phenomena also can be understood at different levels of analysis. The distinction between proximate and ultimate explanations, as Mayr (1982) has argued, is particularly important here. Biological phenomena will have both a proximate explanation in terms of physiological and/or cognitive processes, and an ultimate one by way of the relevant phylogenetic and ontogenetic pathways. A full understanding of any phenomenon in biology must advert to both proximate and ultimate causes. It is one of the main roles of global research programmes, as I proposed in chapter one, to make sense of and integrate

[1] Or, more precisely, evolutionary theory is our *best* explanatory account. Creationist theories also attempt to explain biological phenomena, but provide, as Thagard (1992) clearly illustrates, an account which suffers in comparison to evolutionary theory in terms of global explanatory coherence.

these different levels of explanation. Our explanatory narratives therefore, although both spatially and temporally smeared, should ideally manifest a high degree of overall coherence.

In the first part of this chapter I examine in more detail the structure of the evolutionary research programme in terms of the model of global research programmes detailed in chapter one. This presentation is necessarily incomplete, given the vastness of the programme in question, however it provides a useful sketch of the way explanations are conferred on biological phenomena. In the rest of the chapter I examine the ways in which the general evolutionary research programme has been employed to explain phenomena in the domain of psychology. It is clear here that the various attempts to explain human characteristics from an evolutionary perspective are framed very much in terms of the *general* evolutionary research programme.

I begin by providing a sketch of the history of evolutionary ideas in psychology, with a focus on explaining the reasons behind the acceptability of evolutionary explanations at various times in the programme's history. I then examine the structure of the current evolutionary programme in psychology and discuss some of the different approaches that have been used to explain psychological phenomena from an evolutionary perspective. Although there appears to be considerable disagreement over just *how* to apply evolutionary theory to psychology, I will argue that these differences have been somewhat overstated. I consider that these differences reflect a healthy, but not too divergent, plurality of opinion over several key aspects of the generalised evolutionary research programme that I outline at the start of the chapter.

The structure of the evolutionary programme

There is a large, rich, and diverse literature on the structure of evolutionary theory (e.g., Hull, 1974; Mayr, 1982; Brandon, 1990; Sober, 1993a). Most of this literature focuses on the principle of natural selection itself, the empirical content of the theory, and its explanatory application across a range of domains. My approach differs from these accounts by utilising the model of global research programmes developed in chapter two to examine more broadly the diverse structure of evolutionary theory. As such, my analysis is considerably more coarse grained that those in the literature, and, given the size of the task, offers only a sketch of the important aspects of the evolutionary research programme. My focus here will be on those characteristics of the programme which bear the greatest relevance to my task of evaluating the role of evolutionary explanations in psychology. My account of the evolutionary research programme also focuses on its current state and ignores the evolution of the programme itself [2]. This picture of the programme as it currently stands, of course, is likely to be modified in many respects over time.

The hard core

The hard core of the evolutionary research programme consists of the theory of natural selection itself as independently formulated by Charles Darwin and Alfred Russell Wallace. The principle of natural selection contains three basic conditions required for evolution to occur: variation, inheritance, and fitness (Lewontin, 1970, 1978; Brandon, 1990).

(1) *Variation:* For evolution to occur there must be variation in the distribution of phenotypic characters in a population.
(2) *Inheritance:* These traits must also to some extent be heritable. That is, offspring should be more likely to resemble their parents than other members of the population.
(3) *Fitness:* Different traits must also have fitness consequences, so that those members of a population which possess them are more likely to survive and reproduce than other members of the population who do not.

[2] Hull (1974) makes the distinction between the classical, genetical, and synthetic stages of evolutionary theory. My model is an account of the latter stage, also termed the "Neo-Darwinian synthesis" as it is an integration of theories of population genetics, morphology, systematics, embryology, biogeography and palaeontology (Mayr, 1982).

These three conditions in conjunction with the auxiliary theories of the evolutionary research programme potentially provide the explanatory means to account for all biological phenomena. It is worth noting, however, that the theory of natural selection is more generally a theory of *evolution*. It is entity neutral with respect to *what* is evolving. In this sense Darwinism is truly universal in nature. It should be able to explain evolution on other planets (if it has occurred) regardless of the physical details of the entities involved (Dawkins, 1983). The scope of the various auxiliary theories are, however, more parochial in nature and are likely, in general, to be specific to evolution on this planet.

The protective belt

The theory of natural selection itself has remained virtually unmodified since its articulation by Darwin (1859). However, a range of apparent anomalies to this theory have, in part, been instrumental in the articulation of a large protective belt containing many auxiliary theories, methodologies, and technological processes.

Auxiliary theories

A detailed account of the full panoply of auxiliary theories is far beyond the scope of this analysis. However, a limited selection of theories include the laws of inheritance (both Mendelian and molecular); theories of speciation (allopatric and sympatric); development and embryology (including scaling theory in allometry); and theories of species diversity and distribution (such as the theory of island biogeography). Of particular relevance to psychology is a range of theories, developed predominately since the 1950's including sexual selection theory (Darwin, 1871), inclusive fitness theory (Hamilton 1964a, 1964b), reciprocal altruism (Trivers, 1971), parental investment theory (Trivers, 1972), the theory of parent-offspring conflict (Trivers, 1974), sperm competition theory (Parker, 1982; Smith, 1984), gene-culture coevolution theory (Lumsden & Wilson, 1981) and dual-inheritance theory (Boyd & Richerson, 1985). In explaining biological phenomena auxiliary theories are invoked in conjunction with the theory of natural selection. For example, in explaining the altruistic characteristics of eusocial insects, which seem anomalous from a classical selectionist perspective, the theory of kin selection is employed. From this perspective, sterility and self-sacrifice can be seen as benefiting the individual via differential replication of close kin, and therefore the genes responsible for the altruistic behaviours.

Methodologies and Technologies

The evolutionary programme deploys a wide range of diverse methodologies supported by appropriate use of technology. Many of the methodologies used are more general to scientific inquiry such as the range of statistical and experimental procedures. Advances in methodologies and technologies, such as those used in DNA analysis and in dating techniques, have led to an increased understanding of a range of biological phenomena and prove to be an integral part of the programme's continuing scientific progress. The realisation that humans and chimpanzees shared a common ancestor between five and six million years ago, for example, has led to a radical rethinking of hominoid evolution (Hasegawa, Kishino, & Yano, 1989).

Of particular relevance to an explanation of psychological phenomena is the comparative method in evolutionary studies (e.g., Harvey & Pagel, 1991). By examining the cross-species incidence of a given character across a range of taxa, evolutionary trends can be identified and functions can be elucidated (Coddington, 1988). These studies themselves draw on both molecular dating techniques and DNA analysis to help establish the nature of the phylogenetic relationships under investigation.

Domains of intellectual inquiry.

The domain of the evolutionary research programme could be said to correspond roughly to the domain of living organisms conceived at various levels of structural organisation from molecules to ecosystems. Only roughly of course, because as I noted above, the theory of evolution is neutral with respect to the medium of the evolving entities. However, the full panoply of auxiliary theories, methodologies, and so on are likely to be restricted in application to living systems. There may also be some aspects of living organisms which could be said to fall outside of the scope of the evolutionary programme, however widely conceived. For example, explanations of many human characteristics have traditionally been seen to lie outside of the explanatory purview of evolutionary theory. In particular social behaviour and human culture are often conceptualised as emergent properties of human systems which defy evolutionary analysis. I examine the role of evolutionary explanations in explaining social and cultural phenomena in chapter nine.

Social features

Because of the vast scope of the evolutionary programme, the scientific communities which investigate biological phenomena are widely distributed across different institutions and investigate a diverse range of different kinds of phenomena. Molecular biologists, behavioural ecologists, palaeontologists, and psychologists could all be said to have some allegiance with the evolutionary programme. This distribution of interests often leads to a lack of effective communication between participants and can provide seemingly different ways of understanding the nature of evolution itself.

For example, the palaeontologist Niles Eldredge (1995) draws a division between the views of geneticists, or as he terms them "Ultra-Darwinists", such as George Williams and Richard Dawkins, and those of palaeontologists or "naturalists", such as Stephen Jay Gould, Elizabeth Vrba, and Eldredge himself. This apparent division seems partly a consequence of the different perspectives afforded by palaeontology and genetics, and also perhaps the role of different political allegiances; but is certainly fuelled by media (mis) representation and the participants own, at times exaggerated, critiques of opposing positions.

Within any given field of the evolutionary programme there is also the usual share of disagreement, controversy and social manipulation. David Hull (1988) in his book *Science as a process* provides a particularly illuminating account of the somewhat acrimonious dispute between taxonomists. Similarly, Watson's (1967) autobiographical account of the discovery of the structure of DNA reveals the social nature of science in its competitive manifestations.

Relations to other research programmes

Like all global research programmes, the evolutionary programme has rich connections to many other theories at various levels of globalness. The methodology employed in experimental analysis clearly appropriates theories from the mathematical sciences, while explanations of many biological phenomena are augmented by reference to theories in geology and astronomy. The variation and distribution of species, for example, is in part a consequence of geological processes such as plate tectonics (Hallam, 1983), and global recycling of elements important for life (Lovelock, 1988). The importance of meteor collisions on the earth's evolutionary history is becoming increasingly apparent (Raup, 1991), and is duly informed by theories in the relevant branches of the physical sciences. In explaining many psychological and social phenomena it is also likely that the evolutionary research programme would need to draw on the various specific theories developed by psychologists and sociologists.

Technological applications and social implications

Like all research programmes the evolutionary programme has a rich and complex relationship with the development of technology and with society more generally. The fruits of applied biology are a pervasive part of our society in terms of selective breeding, genetic engineering, medical techniques, and environmental control. The ethical implications of developments in the evolutionary programme are diverse and widespread. The use of evolutionary theory to buttress the philosophy of eugenic movements is amply documented (e.g., Gould, 1981), while the rapid development of the human genome project prompts a host of ethical questions of immediate concern to human welfare (e.g., Kitcher, 1996; Kevles & Hood 1992).

The very acceptance of evolutionary theory was, and is, hampered by a social inertia, fostered in part by religious doctrine, and more generally by the challenge Darwinian theory poses to our uniqueness as a species. That we are resistant to accept our close evolutionary affinity to other species is evident, I think, in the way that humans treat other animals, and our perceived moral obligations to them (Singer, 1995; Rachels, 1990). Another impediment to the acceptance of evolutionary theory is the past misuse of the theory to found eugenic or social Darwinist perspectives. This fear is particularly evident in the social sciences where the development of evolutionary theories of human behaviour have been hampered by claims of sexism, racism, and classism (e.g., Rose, Kamin & Lewontin, 1984; Sahlins, 1976).

The evolutionary programme in psychology

It should be clear that attempts to explain psychological phenomena from an evolutionary perspective are clearly ground in the general evolutionary research programme. Labels such as sociobiology and evolutionary psychology, therefore, refer to those specific parts of the general evolutionary research programme which are employed to explain social and psychological phenomena rather than being global research programmes in their own right. Attempts to explain psychological phenomena from an evolutionary perspective might be considered as attempts to extend the explanatory breadth of the evolutionary programme (Sober, 1993a). As will become clear, however, the use of evolutionary theory to explain aspects of mind and social behaviour in both humans and animals has been present since Darwin's first formulation of the theory of natural selection.

Historical overview

The publication of Darwin's (1859) *The Origin of Species by Means of Natural Selection* may reasonably be said to mark the birth of the evolutionary programme in psychology. Although the *Origin itself* barely alluded to the implications of natural selection for human psychology,[3] it was to provide the logical foundations for the future development of evolutionary explanations of psychological phenomena. Those implications were to be spelled out in explicit detail by Darwin (1871) himself twelve years later in *The Descent of Man and Selection in Relation to Sex,* but were taken up more immediately by Lyell (1863) and Huxley (1863) from somewhat contrary perspectives. Whereas Huxley (1863) clearly saw the importance and relevance of natural selection for explaining the origin and characteristics of humans, Lyell (1863) argued that the principle of natural selection, although possibly true of other animals, was not so of man.

The acceptance of Darwin's theory of natural selection can be seen in part as based on its capacity for explanatory unification (Thagard, 1992). The mass of biological detail accumulated and presented by Darwin in the *Origin* can be seen as *best* explained by invoking evolution by natural selection. As 'Man' is also clearly a biological entity, his characteristics should also be able to be explained via the principles of natural selection. Darwin begins his argument in the *Descent of Man* by explicitly noting the similarities between humans and other animals in terms of their body structure, their nervous system, and their pattern of embryological development. It seemed clear to Darwin that if Man's physiological nature could be seen as continuous with other animals and therefore explicable in terms of natural selection, then his psychological characteristics must also be open to the same kinds of explanations. The idea of continuity between humans and other animals in their mental faculties was crucial to Darwin's argument here and bespoke of a gradual evolution of mind in humans from a non-human ancestor.

[3] Darwin omitted drawing out the implications of his theory for humans except in one brief but illuminating paragraph. "In the future I see open fields for far more important researches. Psychology will be securely based on the foundations already well laid by Mr. Herbert Spencer, that of the necessary acquirement of each mental power and capacity by gradation. Much light will be thrown on the origin of man and his history." (Darwin, 1859, p. 373).

> If no organic being excepting man had possessed any mental power, or if his powers had been of a wholly different nature from those of lower animals, then we should never have been able to convince ourselves that our high faculties had been gradually developed. But it can be shewn that there is no fundamental difference of this kind. (Darwin, 1871 p. 445).

To support his argument of mental continuity Darwin provided a wealth of data on the behaviour of other animals and their putative mental abilities. Attention, insight, memory, reasoning, and the full panoply of emotions, Darwin argued, can be seen, in rudimentary form at least, in other species. Darwin did not deny that there were real differences between humans and other animals, especially in their power of reasoning and in their moral sense; however, these differences were conceptualised as being quantitative rather than qualitative in nature. "Nevertheless the difference in mind between man and the higher animals, great as it is, certainly is one of degree and not of kind" (Darwin, 1871, p. 494) Moreover, the difference between man and ape in intellectual ability, argued Darwin, is certainly less than that between ape and ant. The psychological characteristics of man therefore can not be viewed as being so unique as to require a different *kind* of explanation. Psychology as a science, therefore, could be said to be subsumed by biology. As Darwin succinctly expressed it in his notebooks "He who understands baboon would do more toward metaphysics than Locke".

The idea of psychology as a biological science was developed further in the works of Romanes (1881, 1888) and Francis Galton. Romanes sought to advance Darwin's biopsychology by developing a broad picture of evolution across species so as to trace the genesis of mind. To this end he provided, in *Animal intelligence* (1881), a densely packed volume of anecdotes interspersed with the odd experiment, to demonstrate the grades of intelligence from protozoa to primate. For Romanes, like Darwin, there was no difference in *kind* between animal and human intelligence. Psychology therefore, could be viewed as part of a general enterprise to explain the properties of mind in nature via evolution by natural selection. Galton's work on hereditary genius furthered the explanatory power of the evolutionary programme in psychology by demonstrating that human mental and moral faculties could be passed on to subsequent generations. The mind therefore could be seen to function like any other biological trait, obviating the need for appeal to supernatural sources to explain human intellectual capacities (Richards, 1987).

However, despite what might be been termed a flourishing evolutionary programme in psychology in the latter half of the nineteenth century, public opinion and the views of some of the central figures in the debates over evolutionary theory brought in to question the validity of explaining *human* characteristics via the principle of natural selection. Evolutionary theory itself was widely accepted in Darwin's lifetime, however, the idea that humans and the full array of their psychological and moral faculties were also subject to the principles of natural selection did not become part of the general consensus (Richards, 1987). This explanatory bifurcation of human and other animals remains a familiar theme in contemporary writings, as epitomised in a recent book by the philosopher David Stove (1995, p. vii.):[4] "My object is to show that Darwinism is not true: not true at any rate, of *our* species. If it is true, or near enough true of sponges, snakes, flies, or whatever, I do not mind that. What I do mind is, it being supposed to be true of man"

Part of the reluctance to extend the explanatory schemes of evolutionary theory to human psychology must be attributed to a kind of moral repugnance, felt by some, at the suggestion that humans are, in their fundamental aspects, the same as other animals. The more thoughtful challenges to the extension of evolutionary theory to human characteristics, however, can be viewed as questions regarding the explanatory coherence of the evolutionary programme. In particular, 19th century critics argued that evolutionary theory does not provide an *adequate* explanation of human psychological phenomena.

[4] It is probably worth noting here that Stove is *not* a creationist.

The notion of progress, central to many evolutionary thinkers, in particular Herbert Spencer, provided a challenge to the possibility of the evolution by natural selection of human mental faculties. If the unfit, in intellectual and moral terms, breed more than their psychological superiors (as they appeared to do in Victorian England), then how could evolution have fashioned the superior intellectual virtues of the civilised European?[5] (Richards, 1987). Although to contemporary readers this argument is predicated on the pervasive racist views of the time, it provides an at least plausibly logical problem for evolutionary theory. The problem here is one of empirical adequacy. The theory of natural selection did not appear to explain the pattern of intellectual ability and fecundity actually found in nature, at least not in human nature. The confusion here, of course, is between equating the attributes that a particular social milieu value with those valued by natural selection.[6]

A further challenge to the explanatory breadth of evolutionary theory was provided, much to Darwin's chagrin, by the co-discoverer of natural selection himself, Alfred Russell Wallace (1891). Wallace, in later years, became interested in the spiritualist views of the time, and though admitting that the physical qualities of man were shaped by natural selection[7], suggested that other forces must have been at work in the shaping of the human mind. As Gould (1980c) and Richards (1987) make clear, however, this alternative explanation for the human mind can not be attributed entirely to Wallace's flirtation with the spiritual world. Wallace was much taken with the latent capacities of the human mind as manifested in the ability of 'savages' to attain the accomplishments of civilised Europeans. It seemed to Wallace that human capacities far exceeded the needs

[5] This argument was advanced in a forceful manner by the Scotsman William Greg in an article titled "On the failure of natural selection in the case of man". Greg could not conceive how the superior virtues of say the civilised Scotsman could have been selected for given lower rates of fecundity than their less intellectually able neighbours. " The careless, squalid, unaspiring Irishman, fed on potatoes, living in a pig-sty, doting on superstition, multiplies like rabbits or ephemera:- the frugal, foreseeing, self-respecting, ambitious Scot, stern in his morality, spiritual in his faith, sagacious and disciplined in his intelligence, passes his best years in struggle and in celibacy, marries late, and leaves few behind him" (Greg, 1868, quoted in Richards, 1987, p. 173).

[6] Although a similar version of this problem has also arisen in contemporary debates. As the data collected by Vining (1986) illustrate, those individuals of high status and possessing material wealth actually leave fewer descendants, or at least have fewer children, than less wealthy individuals.

of survival dictated by evolutionary theory. Of what use, Wallace asks, is the refined abilities for music or the aesthetic appreciation for art in the struggle for existence. "We are . . . driven to the conclusion that in his [man's] large and well-developed brain he possesses an organ quite disproportionate to his actual requirements - an organ that seems prepared in advance, only to fully utilised as he progresses in civilisation." (Wallace, 1891, p. 193). As natural selection can only select for immediate benefit, the latent capacities of the human mind must have been provided by alternative means. From the perspective of the theory of explanatory coherence (Thagard, 1992), evolutionary theory, according to Wallace, did not provide adequately coherent explanations of psychological phenomena. The general point made by Wallace, although not his supernatural solution, is as we shall see at the heart of many contemporary criticisms of the evolutionary programme in psychology (see especially Gould, 1991c), and can be seen as a challenge to the explanatory breadth of the evolutionary programme.

Another line of criticism, present in both contemporary and historical discussions, challenges the use of comparative methods to develop an evolutionary psychology of mind. The challenge here is to the use of analogical reasoning to buttress evolutionary explanations of human psychological phenomena.[8] Because the use of analogy plays an important role in the overall explanatory coherence of a research programme (Thagard, 1992) these criticisms served further to question the acceptability of evolutionary explanations in psychology. For Charles Lyell (1863) in the *Antiquity of Man* the superior intellectual and moral qualities of humanity suggested a placement for man in a unique kingdom apart for the rest of organic life. Lyell, a gradualist in geology, whose ideas had greatly influenced Darwin himself, could not comprehend how morality, language, and other mental faculties could possibly arise through *gradual* evolution. For Lyell,

[7] Although he did question the likelihood that humans' lack of hair and the nature of their hands and feet could have been fashioned by natural selection.

[8] I refer to this form of reasoning as analogical in nature even though Darwin and Romanes were seeking to establish evidence for homologous traits. The style of the argument typically proceeded from an analogy from a human trait to similar characteristics found in other animals. "Therefore, having full regard to the progressive weakening of the analogy from human to brute psychology; as we recede through the animal kingdom downwards from man, still, as it is the only analogy available, I shall follow it throughout the

Darwin's extensive use of analogical reasoning simply did not hold; the differences between humans and other animals is simply too great.

The anecdotal method used prominently by Romanes and others as evidence of mental homologies in humans and other animals was called into question by Conwy Lloyd Morgan (1894), who, in his famous canon, exhorts the comparative psychologist not to over interpret an animal's behaviour as evidence of "a higher psychical faculty" if it could be explained through simpler processes. Lloyd Morgan, who argued that such abilities could not occur in the absence of language, disputed the kind of abstract reasoning skills attributed to other animals by Romanes.[9] The extension of evolutionary theory from animals to humans, therefore, especially in the mental realm, was seen as problematic.

Despite these various criticisms of the evolutionary programme in psychology, it would be fair to say that in both England and America evolutionary accounts of human mind and behaviour continued to receive considerable support by both psychologists and philosophers. In America for example, William James, Mark Baldwin, John Dewey, Charles Peirce and others, all utilised Darwinian thinking in various ways to develop theories of mind and behaviour consonant with the general programme outlined by Darwin himself. The classic text in social psychology at this time, which had widespread influence more generally in the social sciences, was William McDougall's (1908) *Social Psychology*. McDougall explicitly drew on Darwinian theory to develop an instinct centred psychology firmly couched in the comparative method.

The decline of evolutionary thinking in psychology from the early part of the twentieth century to its re-emergence in the 1960's can be traced to a variety of causes. Some historians of science such as Degler (1991), and to a lesser extent Richards (1987), focus on the ideological reasons behind the perceived inadequacy of the evolutionary

animal series."(Romanes, 1883, p. 9) Romanes also explicitly utilised a model of evolution based on grades of intellectual achievement regardless of strict phylogenetic relationships.

[9] In later years Lloyd Morgan somewhat softened his views on animal mentality and added in the second addition of his Introduction to comparative psychology that the cannon " by no means excludes the interpretation of a particular activity in terms of higher processes" (1903, p. 59).

programme in psychology. "What the available evidence does seem to show is that ideology or a philosophical belief that the world could be a freer and more just place played a large part in the shift from biology to culture." (Degler, 1991, p. viii) Darwinian thinking in the social sciences, according to Degler, became increasingly associated with the doctrine of social Darwinism and its implications for the eugenic movement championed by Galton and others in the latter half of the nineteenth century. If one accepted evolutionary theory, the promise of progress it was considered, could only be achieved via the selective weeding out of the inferior and the unfit. By 1930 this idea led to the passing of sterilisation laws for criminals and mental defectives in over thirty states in America. Ethical qualms over this widespread sterilisation programme and growing uneasiness about the rise of nazism and its even more draconian methods for assuring racial purity, made social Darwinism, and by implication evolutionary theory, increasingly unpalatable. In terms of the model of global research programmes presented in chapter one, the relations between the evolutionary programme and society were such as to make evolutionary explanations less acceptable, irrespective of their overall epistemic value. Social science turned accordingly from instinct to culture as a means for explaining human characteristics.[10]If individual learning and the social environments were viewed as the major determinants of behaviour then the American egalitarian dream could be better realised (Degler, 1991).

Although it is clear that social factors did play an important role in the general shift away from evolutionary explanations in psychology and the other social sciences, I think more general questions of explanatory coherence played a pivotal role in this change. Evolutionary explanations of psychological phenomena by the turn of the century were seen as becoming increasingly less satisfactory.[11] Much confusion was engendered over the role of instinct in the explanation of psychological and behavioural characteristics. L.

[10] This shift in explanatory focus occurred roughly simultaneously in both psychology and anthropology. Psychologists began to focus more on the learning trajectory of organisms, while anthropologists turned their attention away from human universals to a study of cultural differences in thought and behaviour (Degler, 1991; Richards, 1987).

[11] It is also worth noting that the acceptance of Darwinian evolutionary theory was somewhat on the wane more generally speaking. It was not until the emergence of the evolutionary synthesis in the late 1930's that Darwinism received widespread acceptance in biological circles (White & Gribbin, 1996; Mayr, 1982).

L. Bernard (cited in Richards, 1987) for example, catalogued 1594 different classes of instinct attributed to humans and other animals. The development of behaviourism saw a rapid attenuation of this class of instincts to a few basic processes.[12] Although behaviourism did not deny that there were some innate components to behaviour, the explanatory burden shifted from an analysis of instincts shaped by natural selection to the reinforcing contingencies of the environments and the developmental trajectories of organisms. The explanatory breadth of evolutionary explanations, therefore, became substantially limited. Moreover, the logical empiricist philosophy of science adopted by the behaviourists suggested that science should limit itself to observable phenomena. As the minds of other animals are not open to direct observation, mental concepts should be expunged from the comparative psychologist's vocabulary. By the time Maier and Schneirla (1935) had published their classic text on animal psychology, the study of the animal mind was no longer a legitimate domain of inquiry in mainstream psychology.

We can see here the culmination of two persistent challenges to the role of evolutionary explanations in psychology. Firstly, evolutionary explanations were seen as lacking explanatory breadth in that it was considered that they could only furnish accounts of the few basic innate processes postulated by the behaviourists and therefore could not play an important role in explaining the full range of behaviour exhibited by humans (and other animals). And secondly, the important source of analogy drawn on by Darwin, Romanes and others, was seen as an illegitimate form of inquiry. Coupled of course with these two challenges was the rise of a serious competing research programme, behaviourism. Behaviourism promised, at least, an increase in explanatory unification of psychological phenomena via the invocation of a few basic learning processes and the contingencies of the environment.[13]

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[12] Indeed, between 1928 and 1958 listings of instinct in the psychological abstracts compared to reinforcement and motivation reduced from 68% to 8% (Herrnstein, 1974).

[13] The hegemony of purely learning explanations in psychology for most of this century is nicely illustrated by the teaching of students in the 1950's that spiders *learn* to make their webs. (Jim Pollard, personal communication)

Ethology and the emergence of sociobiology

The publication of E. O. Wilson's (1975) *Sociobiology: The new synthesis* can be seen as a pivotal work in the re-emergence of evolutionary thinking in psychology and the social sciences. Wilson's massive volume not only provided a comprehensive review of the evolutionary basis of social behaviour in animals, but also extended this evolutionary approach to explanations of human behavioural patterns. From Wilson's perspective, psychology, sociology, and anthropology could all be viewed as part of the general evolutionary synthesis.

However, despite the importance of Wilson's work, an increasing interest in biological approaches to psychology and the other social sciences was beginning to emerge as early as the late 1940's in America (Degler, 1991). This increasing interest in the evolutionary antecedents of behaviour in Anglo-American psychology can be traced, in part, to the development of ethology in Europe, epitomised in the work of Niko Tinbergen and Konrad Lorenz. Whereas behaviourists focused their attention on the behaviour of animals (predominately rats and pigeons) in controlled laboratory environments to various contingencies of reinforcement, ethologists were more interested in the behavioural repertoire of animals as it occurred in their natural environment. In a long series of studies carried out from the 1930's onwards, Lorenz (1971) and Tinbergen (1973) demonstrated, contrary to behaviourist doctrine, that animals possessed a large number of species-specific instincts which give rise to adaptive behaviours under appropriate (and sometimes inappropriate) environmental conditions. Although not entirely rejecting learning as an explanation for behaviour, the focus of Lorenz and Tinbergen was on behaviour which could occur in the absence of specific environmental contingencies and which seemed to emerge without periods of prior conditioning.[14] An explanation of these instinctual behaviour patterns was sought in evolutionary theory.

[14] Hence the emphasis on so-called deprivation studies. If a behaviour pattern was seen to emerge even when an animal was raised in isolation, it was argued that the source of that behaviour must be something internal and built into the animal, not a consequence of learning.

Certain patterns of behaviour, like the imprinting of geese, the egg fanning of sticklebacks, and the egg shell removal routines of black-headed gulls, were viewed as behavioural adaptations which had been selected for their ability to increase the reproductive success of the animals in question.

Both Lorenz and Tinbergen clearly indicated that this evolutionary approach to animal behaviour could be extended to an analysis of *human* behavioural patterns. Tinbergen (1973) stressed the importance of comparative analysis and rejected any *a priori* assumption that humans were *essentially* different from all other animals. Likewise, Lorenz (1971) pushed for a comparative approach to human psychology and argued that an evolutionary perspective was crucial in providing explanations of mind and behaviour.

> The unquestionable and unquestioned fact of evolution automatically leads to recognition of the corollary fact that an enormous number of structural properties of human behaviour and the human mind owe their particular nature to unique historical pathways of phylogeny. Without knowledge of phylogenetic relationships, these features must remain incomprehensible.
> (Lorenz, 1971 p. 243.).

The approach of Lorenz and Tinbergen was to have a substantial influence on subsequent developments in psychology, anthropology and sociology throughout the English speaking world. However, the reaction of American biologists and psychologists was not entirely favourable. Both Beach (1955) and Lehrman (1953) challenged the usefulness of the concept of instinct for explaining animal behaviour. Lehrman (1953), in a vehement critique of Lorenz's instinct theory, argued that it was simply not possible to demarcate what is learned from what is innate. Isolation experiments, according to Lehrman prove nothing, for it is conceptually impossible to entirely isolate an organism from its environment. Lehrman argued further that the use of analogy from animal to human was simply inappropriate; similar outcomes in behaviour need not bespeak similar causes.

The relative contribution of innate causes and learned causes, of course, has been, and continues to be, a source of much debate among psychologists, sociologists, and anthropologists. Lorenz's reply to the objections raised by Lehrman and others was to point out that the fact of learning was not denied, but the *way* learning was achieved could not be adequately explained from a behaviourist perspective. The learning of organisms, Lorenz argued, is *directed* in nature in a way that is only explicable from an evolutionary point of view (Richards, 1974). In the words of the contemporary biologist Peter Marler (1991, p. 37), organisms have an "instinct to learn".

This insight, that learning is directed in a way explicable by evolutionary theory, was to have far reaching implications for developments in Anglo-American psychology, where the hegemony of behaviourism was beginning to be challenged. One source of disillusionment over the value of the behaviourist approach to research was in the highly *un*comparative nature of their comparative research (Beach, 1965; Bitterman, 1960). Over half of all studies prior to 1950 were carried out on a single, by now highly inbred species: the Norway rat. The rat was simply viewed as a convenient organism whereby the universal laws of learning might be realised. As Beach (1965, p. 10) queried in 1950: "Are we building a general science of behaviour or merely a science of rat learning?"

The behaviourist approach also ignored questions regarding the evolutionary origin of behaviour, and in particular eschewed instinctual explanations for animal behaviour patterns. The laws of learning, as formulated by the behaviourists, assumed that virtually all learning was via association (of some kind), and that all stimuli to be associated are equally meaningful to the organism. The rejection of instinctual behaviour and the assumption of the equipotentiality of stimuli, however, were beginning to be questioned from a variety of sources.

Chomsky's (1959) influential review of Skinner's *Verbal Behavior* critiqued the idea that language was learned and controlled by the contingencies of reinforcement exercised by the verbal community. Chomsky explicitly drew on the work by ethologists such as

Lorenz and Tinbergen, which demonstrated that organisms have a propensity to learn in certain directions at certain times of their life. Chomsky (1959, p. 57) concluded that behaviourism simply provided an inadequate explanation for the phenomenon of language learning:

> The fact that all normal children acquire essentially comparable grammars of great complexity with remarkable rapidity suggests that human beings are somehow specifically designed to do this, with data-handling or hypothesis-formulating ability of unknown character and complexity.

The important role of instinct in explaining behaviour was also demonstrated by Breland and Breland (1961), who in the course of training animals using operant and classical conditioning techniques ran in to a certain kind of problem. Breland and Breland found that after a period of operant conditioning, animals would start to display non-conditioned behaviour and would not continue with the task for which they had been trained. Pigs for example, which had been conditioned with food reinforcers to transfer dollar coins in to a 'piggy bank' would start dropping the coins to root at them and toss them around. In short, the pigs seemed to be displaying the instinctual behaviours associated with natural foraging. Breland and Breland (1961, p. 684) termed this phenomenon 'instinctual drift' and concluded:

> After 14 years of continuous conditioning and observing thousands of animals, it is our reluctant conclusion that the behaviour of any species cannot be adequately understood, predicted, or controlled without knowledge of its instinctive patterns, evolutionary history, and ecological niche.

Through a number of experiments it also became clearly apparent that certain behaviours are more readily acquired and more easily associated with certain classes of stimuli than others, thereby contravening the equipotentiality assumption of the behaviourists. Seligman and Hager (1972) argued that organisms bring with them into the laboratory not only a history of learning but also an evolutionary history. Animals, Seligman and Hager suggest, are 'prepared' to learn some things easily but not others. Garcia and colleagues (Garcia & Koelling, 1972; Garcia, Ervin & Koelling, 1972) demonstrated this phenomenon of 'preparedness' in an elegant series of studies. Garcia demonstrated that rats more readily associate radiation-induced nausea with food than they do with other stimuli such as electric shocks or lights, whereas audio-visual stimuli were more easily paired with foot shocks than with food. This kind of prepared learning was also manifest even after long delays between the pairing of the food stimulus and the subsequent nausea, challenging the notion that recency is an important criterion in accounting for the strength of the association.

These challenges to behaviourism as an explanatorily adequate account of animal and human behaviour was clearly an important part of the return of evolutionary theory to the psychological sciences. In addition to this repudiation of some of the central behaviourist assumptions was a development of a range of new theories in evolutionary biology which provided a conceptual means to expand the explanatory scope of the evolutionary programme in psychology.

In a pair of classic papers, Hamilton (1964a, 1964b) augmented the core assumptions of natural selection with his theory of kin selection. By arguing that selection can favour behaviour which increases the reproductive success of one's kin, Hamilton provided a means to explain the altruistic behaviour of many animal species, and hence increase the explanatory breadth of the evolutionary programme. Additional theories developed throughout the 1970's such as parental-investment theory (Trivers, 1972), reciprocal altruism (Trivers, 1972), parent-offspring conflict theory (Trivers, 1974) and game theory (Maynard-Smith, 1974) provided further explanatory resources for the evolutionary programme. The important consequence for psychology and the other social sciences of these conceptual developments was the potential for satisfactory explanations to be articulated for *social* behaviour. Accompanying these theoretical developments was also a dramatic increase in the number of naturalistic studies of animal (and importantly primate) social behaviour documented in some detail in Wilson (1975). As a consequence, by the time Wilson's (1975) *Sociobiology* was published there existed a coherent body of theory and empirical work that could be used to develop potentially satisfactory explanations of human behaviour from an evolutionary framework.

Wilson is often, perhaps justly, criticised (e.g., Kitcher, 1985) for writing two separate books in *Sociobiology*; one, a careful, detailed and elaborate account of animal social behaviour from an evolutionary perspective, and the other a highly speculative and unsubstantiated evolutionary view of human social behaviour. Wilson (1975, p. 3) defined sociobiology as " The systematic study of the biological basis of all social behaviour" and demonstrated to great effect the power of evolutionary theory, and in particular the newly developed theories of Hamilton and Trivers, to explain patterns of social behaviour among a wide variety of animal species. Like many earlier attempts to explain patterns of human behaviour with evolutionary theory, Wilson drew strongly on arguments from analogy (and homology) with other species, and emphasised the unifying power of the evolutionary programme. Wilson, in his detailed studies of ant societies was impressed by the strong convergences of social behaviour between a wide variety of species, including humans (Wilson, 1994). The social sciences, according to Wilson, were ripe to be absorbed using the newly emerging principles of sociobiology, embedded as they were in the more general evolutionary research programme.

> . . . sociology and the other social sciences, as well as the humanities are the last branches of biology waiting to be included in the modern synthesis. One of the functions of sociobiology, then, is to reformulate the foundations of the social sciences in a way that draws these subjects into the modern synthesis.
> (Wilson, 1975 p. 4).

Evolutionary theory therefore, according to Wilson, could provide the most explanatorily unifying account of social behaviour across species, including humans.

The early sociobiological programme, however, as developed by Wilson (1975, 1978) in respect to human phenomena, often proceeded in the speculative mode. Wilson's (1975, 1978) strategy was to list attributes of human behaviour such as aggression, incest avoidance, religion, ethics, ritual, culture, magic, homosexuality, reciprocity and so forth and to argue that they represented genetic predispositions which lead ultimately to increased reproductive success. Typically Wilson's accounts fell very much to the "how possibly" end of the epistemic spectrum and tended to ignore the details of the proximate psychological, developmental and social mechanisms underlying the phenomena in question. Indeed Wilson's early sociobiological writings tended to be somewhat reductionistic in several different senses. Firstly, Wilson provided a strong gene selectionist account of evolution, and secondly, Wilson seemed to advocate a physiological reduction of mind to brain. For example, in *Sociobiology* Wilson (1975 p. 575) suggested that "having cannibalised psychology, the new neurobiology will yield an enduring set of first principles for sociology". As Wilson (1994) notes in his autobiography, he was highly impressed by the success of the reductionistic method in molecular biology and sought a similar approach for the social sciences. As a consequence Wilson's (1975, 1978) early writings on sociobiology tended to leapfrog entirely the psychological level of explanation and advocated physiological explanations of behavioural patterns in humans and other animals. This is perhaps one reason why the impact of sociobiology was greatest initially in sociology and anthropology, rather than psychology.

Indeed, prior to 1980 evolutionary approaches in the social sciences were most prevalent in the anthropological literature where researchers such as Alexander (1979), Chagnon and Irons (1979), and others were applying evolutionary theory to explain patterns of cultural behaviour in traditional societies. By the late 1980's two similar but distinct evolutionary approaches to human behaviour had emerged: Human behavioural ecology and evolutionary psychology. There has been much confusion over just what terms to employ to distinguish the different evolutionary approaches to the human sciences and just how they differ in their theoretical perspectives. I will examine some of the similarities of the different approaches in more detail below, but I think we can make rough sociological, epistemological and methodological distinctions between human behavioural ecologists on the one hand and evolutionary psychologists on the other. Blurton-Jones (1990) also argues that there is a third evolutionary approach to the study of human behaviour - Dual inheritance theory - as characterised in the work of Boyd and Richerson (1985). However, I would suggest that dual inheritance theory be considered an additional auxiliary theory in the evolutionary programme which can be drawn on by both human behavioural ecologists and evolutionary psychologists to explicate patterns of cultural transmission from an evolutionary perspective, rather than a distinct alternative. The terminology employed for evolutionary approaches to human behaviour has been highly variable. Researchers who would have earlier identified themselves as sociobiologists may now prefer to be called human behavioural ecologists or evolutionary psychologists, while other labels such as human ethology, biosociology, socioecology, Darwinian anthropology and Darwinian psychology have also been employed. To simplify matters I will retain the crude, but reasonable distinction between human behavioural ecology and evolutionary psychology.

Human Behavioural Ecology

Human behavioural ecology can be defined as "the study of the evolutionary ecology of human behaviour. Its central problem is to discover the ways in which the behaviour of modern humans reflects our species' history of natural selection" (Cronk, 1991, p. 25) and its focus as Borgerhoff Mulder (1991, p. 69) states is to "determine how ecological and social factors affect behavioural variability within and between populations."

Researchers who identify themselves as human behavioural ecologists (e.g., Borgerhoff Mulder, 1991; Irons, 1979; Crook and Crook, 1988; Alexander, 1979, 1990; Turke, 1990) can be distinguished disciplinarily from evolutionary psychologists in that they tend to be anthropologists whereas evolutionary psychologists typically (but not always) have a background in the psychological sciences. This sociological difference seems to be part of the reason behind the different methodological approaches employed by the two groups.

The focus of human behavioural ecology, therefore, is on detailed ethnographic studies of contemporary human populations with a particular focus on traditional, non-industrial societies. The aim is to demonstrate that the patterns of behaviour detailed in the research can be viewed as adaptive in nature. That is, individuals are viewed as behaving in a way that maximises their reproductive success (Borgerhoff Mulder, 1987). Human behavioural ecologists subscribe to the idea that humans have a highly variable phenotype which exhibits considerable behavioural plasticity. Therefore humans have the capacity to vary their behaviour in different social and ecological circumstances in a manner that leads to inclusive fitness maximisation (Cronk, 1991; Borgerhoff Mulder, 1987, 1991; Alexander, 1990; Irons, 1979a). In particular, it is argued that despite variations in cultural traditions behaviour can adaptively track these variations and that success in cultural terms also tends to leads to Darwinian success in terms of increased reproductive fitness (Irons, 1979). Borgerhoff Mulder (1996, p. 205/206) summarises the approach of the human behavioural ecologists as follows:

> In brief, individuals are viewed as facultative opportunists who assess, either consciously or not, on either the behavioural or the evolutionary time scale, a wide array of environmental conditions (both social and ecological) and determine the optimal fitness-maximising strategy whereby they can out compete conspecifics in terms of the number of genes transmitted to subsequent generations.

This flexibility in behavioural response is emphasised in the work of Alexander (1979; 1990) who argues that humans have evolved a capacity - consciousness - which allows them to develop scenarios in a way that which helps them to respond adaptively to changes in social and ecological contexts. It is not argued that human *consciously* strive to increase their reproductive fitness, but simply that reproductive fitness is a consequence of their striving for other goals which are highly correlated with reproductive fitness. For example, Napoleon Chagnon's (1988) work on the Yanamomo of South America demonstrates that the culturally valued trait of fierceness correlates with reproductive success. Those fierce members in a group help to prevent warring raids by other groups and obtain elevated status in their own group which leads to an increase in marital and reproductive success.

The emphasis on humans as highly facultative fitness maximisers is clearly seen in the work of Borgerhoff Mulder (1988, 1997), and Crook and Crook (1988). In Borgerhoff Mulder's study of kipsigis bride-wealth payments, it was found that physical correlates of lifetime fertility in women, such as early menarche, engendered greater bride-wealth payments from men. However, when economic conditions changed so that large families became insupportable, this correlation disappeared. Borgerhoff Mulder (1997) concludes that these modifications in mating effort may reflect the underlying assumption of fitness maximisation. Men prefer highly fertile woman when conditions allow for potentially large families, but under changing ecological circumstances this preference becomes less important. These preferences are reflected in the size of bride-wealth payments. In a similar vein Crook and Crook (1988) in their study of Tibetan polyandry argue that polyandry is an adaptive response to specific ecological circumstances associated with low levels of subsistence and little opportunity for expansion and dispersal.

The methodological approach employed by human behavioural ecologists is highly similar to that used in the study of other animals. Optimal patterns of behaviour are derived from a consideration of natural selection and the auxiliary theories of the evolutionary programme. Extant populations are studied and reproductive success is measured. How well the evolutionary programme predicts and explains differential patterns of reproductive success gauges the success of any particular study.[15]

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[15] Although I have outlined this basic methodology in sequential terms, research may start at any point in this sequence. For example, the detailed collection of data may proceed prior to the development of specific evolutionary hypotheses.

Inclusive fitness theory and the theory of reciprocal altruism, for example, predict that patterns of food sharing should be different depending on the degree of genetic relationship between individuals, and on past instances of food sharing. Research on food sharing on Ifaluk (Betzig & Turke, 1986) and among the Ache of Paraguay (Kaplan & Hill, 1985) have borne out some of these predictions. On Ifaluk food sharing is more frequent among close genealogical kin and individuals take into account specific costs and benefits in their distribution of food (Betzig & Turke, 1986). Among the Ache some sources of food but not others were shared preferentially among kin; meat and honey tended to be shared band-wide, whereas other food sources were shared more among close kin groups. Sharing food also raised the nutritional status among all individuals as the important food sources of meat and honey fluctuated highly in their abundance. This increase in nutritional status tended to reflect band-wide sharing rather than networks of reciprocal alliances. However, individuals who procured more game tended to increase their reproductive success through their overall value to the band (and hence higher status) and through an increase in extra-marital relationships. This finding bears out one of the general predictions of the human behavioural ecological approach: that striving for cultural success (in particular prestige) leads to an increase in reproductive success, even though the specific forms of behaviour may be highly variable (see Hill, 1984).

The key features of human behavioural ecology can be summarised as (1) an anthropological focus on human behaviour in traditional societies; and (2) the way this behaviour represents adaptive responses to changing ecological and social contexts, measured by (3) the degree of reproductive success, or some proxy of, those individuals concerned. Predictions are drawn from the generalised evolutionary research programme and comparative studies are used where necessary to support the various claims that are made.

Evolutionary psychology

Evolutionary approaches to human behaviour have received widespread criticisms from a variety of sources. These criticisms can be roughly partitioned into those directed from outside the evolutionary research programme and those which have emerged from within the programme itself. The approach of the human behavioural ecologists has received a variety of criticisms from an emerging group of researchers who characterise themselves as evolutionary psychologists (e.g., Symons, 1989, 1990; Cosmides & Tooby, 1987, Tooby & Cosmides, 1992; Buss, 1995). Two main interrelated criticisms have been directed at the methodological and conceptual approaches adopted by the human behavioural ecologists.

The first criticism suggests that the focus of the human behavioural ecologists on behaviour is simply too fine grained. Because humans are characterised by a considerable degree of phenotype plasticity it is highly unlikely that specific behaviours will represent evolutionary adaptations (Symons, 1989; Cosmides & Tooby, 1987; Sterelny, 1992). This focus on behaviour follows from what the evolutionary psychologist's see as the inappropriate assumption that humans are inclusive fitness maximisers and that they possess mechanisms with *that* goal (Buss, 1995). As Tooby and Cosmides (1992) argue, humans are not fitness maximisers *per se*, but are *adaptation* executors. There is no simple 'maximise fitness' rule, so the variety of behaviours investigated by human behavioural ecologists, regardless of their apparent adaptiveness may not be adaptations and therefore are not (in any clear cut way) amenable to evolutionary explanations.

The second, related criticism, suggests that measuring reproductive success is an inadequate means of elucidating adaptations. Reproductive success can only tell us about current directional selection, it cannot be used to illuminate adaptive design. Evolutionary psychologists suggest that the human behavioural ecologists have got the approach to identifying adaptations backwards:

> In the study of adaptation, the key issue is not whether a given phenotypic feature influences reproductive success, but rather whether differential reproductive success historically influenced the form of the phenotypic feature.
> (Symons, 1989, p. 137)

The notion of adaptation is a historical concept. If we are to side-step the old tautology argument against natural selection: that the survival of the fittest can be reformulated as the survival of the survivors, then we need to get a handle on adaptive *design* (Williams, 1966; Symons, 1989, 1992). Because the environments that humans inhabit have changed quite considerably since the evolution of *Homo sapiens* our current behaviours, however adaptive in the sense of reproductive success, may be a poor guide to elucidating adaptations. The kind of study undertaken by Crook and Crook (1988), for example, is problematic from the evolutionary psychologist's perspective. As Symons (1990) argues, it is highly unlikely that Tibetan polyandry is an adaptation, because however facultative a species humans may be, natural selection cannot favour designs which are likely to produce adaptive response to the range of novel circumstances faced by Tibetan farmers such as agricultural estates, landlords, taxation, primogeniture and so forth. Symons (1989, p. 139) concludes:

> Polyandry obviously must be underpinned by some array of psychological mechanisms, with specific selection histories, but it is an *adaptation* only if at least one of these mechanisms was designed to specifically to produce it. If no such specialised mechanisms exist, then polyandry is not a Darwinian adaptation, and measuring reproductive differentials among individuals who do and do not marry polyandrously is irrelevant. Reproductive differentials would be significant only if they constituted evidence for the existence of, or nature of, a specialised polyandry-producing mechanism in the human psyche.

This quote from Symons highlights the core concern of a number of authors regarding sociobiology and human behavioural ecology: evolutionary approaches to human behaviour cannot ignore the pivotal role of proximate mechanisms in producing behaviour (Symons, 1989, 1992; Cosmides & Tooby, 1987; Tooby & Cosmides, 1992; Buss, 1995; Sterelny, 1992; Kitcher, 1985, 1990). Adaptations are not seen at the behavioural level, but instead occur at the level of psychological mechanisms. According to Cosmides and Tooby (1987, p. 281) " Natural selection cannot select for behaviour per se; it can only select for those mechanisms that produce behaviour", so " The causal link between evolution and behaviour is made through the psychological mechanism" (Cosmides & Tooby, 1987, p. 277).

The cognitive level of explanation therefore is primary because " *Adaptive behaviour is predicated on adaptive thought:* an animal must process information from its environments in ways that lead to fit behaviour while excluding unfit behaviour" (Cosmides & Tooby, 1987, p. 283). A focus on psychological mechanisms is important, argue the evolutionary psychologists, because this is the level at which invariance emerges. The variability in phenotype expression found across individuals reflects a uniformity in psychological mechanisms in interaction with varying environments (Cosmides & Tooby, 1987; Buss, 1985).

Psychological mechanisms have evolved to solve adaptive problems in what is termed the environment of evolutionary adaptiveness (EEA). The EEA is given a precise definition by Tooby and Cosmides (1996, p. 122).

> The EEA is the set of selection pressures (i.e., properties of the ancestral world) that endured long enough to push each allele underlying the adaptation from its initial appearance to effective fixation (or to frequency-dependent equilibrium), and to maintain them at that relative frequency while other necessary alleles at related loci were similarly brought to near fixation. Because moving mutations

> from low initial frequencies to fixation takes substantial time, and sequential fixations must usually have been necessary to construct complex adaptations, complex functional design in organism owes its detailed organisation to the structure of long-enduring regularities of each species' past.

The importance of understanding psychological mechanisms as adaptations to ancestral environments highlights the potentially problematic nature of looking for adaptations at the *behavioural* level in contemporary environments. Given changes in the environment, the precise forms of behaviour, although underwritten by adaptations in the form of psychological mechanisms, are not likely to be adaptations themselves.

Evolutionary psychologists such as Buss (1995) and Tooby and Cosmides (1992) suggest the humans have evolved a large number of psychological mechanisms that have been selected to solve specific adaptive problems. The cognitive architecture of the mind, therefore, is likely to posses a wide range of domain-specific psychological mechanisms which serve distinct functions. Some examples of psychological mechanisms suggested by Buss (1995) include those which underlie sexual jealousy, fear of snakes, language, landscape preference, mate selection and reciprocal exchange.

Tooby and Cosmides (1989) argue that the importance of reciprocal altruism in hominid evolution would have led to the selection of a wide range of psychological mechanisms underlying reciprocal exchange. One important problem faced by early hominids engaging in social exchange would have been the detection of cheaters: those individuals who take the benefits of an exchange without reciprocating in kind. Tooby and Cosmides (1989) hypothesised that there should be a special mechanism that subserves this adaptive function and which operates in the social domain. They interpret the content effect on the Wason selection task as evidence of this specialised mechanism, or more appropriately, as a phenomenon which is adequately explained by the presence of such a mechanism. The Wason selection task is an exercise in deductive reasoning which involves subjects being presented with four cards. In the abstract reasoning condition each card has a letter on one side and a number on the other. The subjects may be shown the cards E, K, 2, 7, and asked which two cards that they need to turn over to see if the following claim is correct: "If a card has a vowel on one side, then it has an even number on the other." Performance on this task is usually extremely poor. However, when the cards presented have ages on one side and beverages on the other (e.g., beer, coke, 22, 16), and the subjects are asked which cards need to be turned over to determine the rule: "If a person is drinking beer, he or she must be over 19", performance is substantially enhanced. Success on the Wason selection task seems to be content dependent. Abstract problems tend to elicit lower success rates than those embedded in a social contract. This higher success rate, Tooby and Cosmides (1989) demonstrate, is not the consequence of familiarity *per se*, but it is due to the problem being expressed in the form of a social contract.[16]

Other examples of domain specific mechanisms which are suggested to have a distinct evolutionary basis include those underlying language (Pinker, 1994), the attribution of mental states to others (Baron-Cohen, 1995), preference for landscapes (Orians, 1992), mate selection and judgements of attractiveness (Buss, 1994), jealousy (Wilson & Daly, 1992) and spatial ability (Gaulin & Hoffman, 1988).

The argument for domain specificity is both conceptual and empirical in nature. The conceptual arguments for domain-specificity are related to the general problem of induction and learning. In order to narrow the range of possible interpretations of the environment, there need to be ways of organising or framing that information. It has been argued by some (e.g., Cosmides & Tooby, 1994; Symons, 1990; Buss, 1995) that a domain-general architecture cannot, in principle, guide adaptive behaviour. The central point here is that what counts as fit behaviour differs dramatically from domain to domain. When the solution to two adaptive problems are incompatible we would expect the evolution of two specialised mechanisms which are capable of solving those different

[16] Cosmides' social contract theory has been extended somewhat by Gigerenzer and Hug (1992), who clarify the role of cheater detection in the Wason selection task. Social contract theory, however, has not escaped criticism from a variety of sources (e.g., Cheng & Holyoak, 1989 Politzer, & Nguyen-Xuan, 1992).

problems. We see this kind of specialisation, of course, in the physiological architecture of the human body, so we should also expect it at the psychological level. Just as there are specialised organs which subserve the function of pumping blood and excreting waste, so should there be specialised psychological mechanisms dedicated to language processing and to the processing of information involved in object recognition.

Simple trial and error learning using generalised learning mechanisms is too costly a process for organisms to typically burden. As the work of Garcia et al. (1972) and others has demonstrated, organisms are so designed as to be prepared to learn certain associations in the world but not others. A truly domain-general system simply cannot cope with the world's complexity over the trajectory of a single lifetime; instead, evolution is likely to have fashioned organisms with ways of demarcating the world and responding to it in a manner specific to the problems encountered regularly in their environment.

The idea of domain-specific mechanisms, or what in earlier parlance Cosmides & Tooby (1987) termed 'Darwinian algorithms' is tied closely, but is logically distinct from, the idea of cognitive modules, popular in the cognitive science literature. Fodor's (1983) Modularity of mind thesis brought to the attention of psychologists and philosophers the idea that the mind may be organised in terms of a series of perceptual and cognitive modules. A module, in Fodorian terms is a specialised computational device which is innately specified, informationally encapsulated, and which operates in a specific domain on a certain class of inputs. Fodor ,however, limited his modularity of mind to perceptual processes (although he did include language) and argued that conceptual or central processes were non-modular in nature. The approach of the evolutionary psychologists is that the architecture of the mind is overwhelmingly modular in nature. Not only are their specialised modules for perceptual processes, but there are also specialised cognitive modules serving a wide range of higher order functions.

The empirical evidence for domain specificity and for a modular cognitive architecture comes from a variety of sources. Evidence from developmental psychology which suggests the existence of specialised learning mechanisms is found in studies which demonstrate that young infants acquire information about the world and learn new skills in a way not compatible with general learning processes. Evidence here is strongest when the developing skills and knowledge are radically underdetermined by the input from the environment and do not seem to follow from any obvious history of reinforcement. The best example here is language, which emerges rapidly in the absence of any obvious learning history; however, specialised learning mechanisms underlying object perception (Spelke, 1991), the classification of inanimate and animate objects (Gelman, 1990), the development of a theory of mind (Baron-Cohen, 1995), and natural history classification (Atran, 1990) have also been suggested.

Further evidence for the modularity of mind is drawn from the neuropsychological literature. Selective impairment and sparing of specific functions after neurological insult of some kind, suggests the existence of specialised neural machinery which underlie the functions spared or impaired.[17] These kinds of impairment can occur in a wide variety of different systems. Visual agnosias are the result of specific neural damage which leads to inability to recognise objects, with prosopagnosia, the inability to recognise faces as a specific instance of this disorder. Individuals with such agnosias are often quite unimpaired in other perceptual and cognitive domains, suggesting the existence of specialised modules which underlie the function of object and face recognition. Language can also be specifically impaired with focal brain damage to Brocas or Wernickes area, and it has recently been suggested (e.g., Baron-Cohen, 1995) that autism is a specific impairment to the theory of mind module that serves the function of attributing mental states to others. Modular approaches to the mind, however, have also received a fair degree of criticism, and there is no general consensus about just how to characterise

[17] The results from such studies, however, are rarely unequivocal and it is usually difficult to demonstrate any clear one-to-one relationship between specific brain areas and specific functions. Given that parts of the brain typically work in conjunction with one another, and that evolution is gradual in nature, this is perhaps what we would expect. However, the relevant studies, from neuropsychology do often suggest a relative amount of modularity and a degree of autonomy in brain systems.

modularity or domain-specificity. I will discuss some of these issues in more detail below.

The general research strategy of the evolutionary psychologist, then, is to focus on the identification of psychological mechanisms which have evolved to solve specific adaptive problems in the environment of evolutionary adaptiveness. By focusing on the function of the mechanisms concerned, it is argued that we should be able to carve the mind at its proper joints. Cosmides and Tooby (1994) urge that we should study not what the mind *can* do but what it was *designed* to do. Because form follows function, an emphasis on the adaptive origin of psychological mechanisms should also help us to uncover the underlying cognitive architecture of the mind and the way cognitive mechanisms interface with the environment to produce behaviour.

Criticisms of evolutionary psychology: getting the best out of evolutionary theory for psychology

The approach adopted by evolutionary psychologists has received criticism from a variety of sources from within the evolutionary programme itself. Human behavioural ecologists (e.g., Turke, 1990; Smuts, 1990; Alexander, 1990) have defended their methodological and conceptual approach to the evolution of behaviour, and have argued that a focus on psychological mechanisms is inappropriate and the notion of the EEA is vague and unhelpful. Other lines of criticism suggest that evolutionary psychologists have adopted an excessively individualistic approach to human evolution that ignores the importance of social groups (Brewer & Caporael, 1990; Caporael & Brewer, 1991; Caporael & Baron, 1997). Furthermore, this emphasis on individuals, it has been suggested, obscures the possibility of group selectionist arguments for human characteristics.

I shall examine each of these criticisms in turn, argue that the differences between alternative approaches has been exaggerated, and sketch out the way an evolutionary approach to psychology needs to aim for complete explanations which draw on the full resources of the evolutionary programme.

Mind or behaviour?

A crude rendition of the debate between evolutionary psychologists and human behavioural ecologists would be to see it as an argument over whether the psychological or behavioural level of explanation is the appropriate one to look for human adaptations. Both Turke (1990) and Alexander (1990) defend an approach which views focuses on the behavioural level. Alexander (1990) argues that it is behaviour which is directly visible to natural selection and that psychological mechanisms are only selected to the extent that they realise behaviour. Moreover, it is argued that behaviour is easier to study than the underlying mechanisms, which often remain obscure. While Turke and Alexander emphasise the importance of studying behaviour, they also point out that it is the whole system which is subject to selection and hence to adaptation explanations.

This is surely a reasonable point and one on which the evolutionary psychologist would concur. We need detailed evolutionary explanations of psychological, physiological and developmental mechanisms that give rise to adaptive behaviour under the relevant ecological circumstances. This would of course mean that the anthropological studies conducted by the human behavioural ecologists need to focus more on cognitive processes than they have in the past. This follows from the general and valid point urged by evolutionary psychologists, that variable behaviour can be underwritten by invariant psychological mechanisms operating in different environments.

Instances of behaviour, therefore, can only be viewed as adaptations to the extent that they are part of a co-ordinated system specifiable (in principle at least) in psychological, physiological, and developmental terms, which has been selected *for* its effects in given environments. Tibetan polyandry can only be viewed as an adaptation, regardless of how adaptive it is, if there are specific psychological and developmental processes which have evolved to respond to specific environmental cues in a way that leads to the decision to marry polyandrously. In this particular example, like many, the question of adaptation hinges on the nature of both the putative mechanisms and the appropriate environmental cues. These two factors provide avenue for further debate between alternative approaches.

The EEA

Part of the problem evolutionary psychologist's have with studies such as those by Crook and Crook (1988) and Borgerhoff Mulder (1988) is that the behaviour hypothesised as adaptive is elicited in response to what is seen as highly novel circumstances, ones which could not have occurred in our evolutionary history and therefore which could not have been part of specific selective regimes. Part of this problem and subsequent disagreement hinges around the specificity of the features in the environment that humans are responding to. It is surely true that humans do not have specific mechanisms which have evolved to construct certain types of behaviour under conditions of pastoralism, taxation, and so forth; but these features of contemporary environments may reflect more invariantly fluctuating aspects of our ecology such as resource availability, population structure or social mobility.

Just what our ancestral environments were like is a matter for some debate and human behavioural ecologists argue that the notion of the EEA as used by evolutionary psychologists is simplistic and ill-defined (Borgerhoff Mulder, 1991). I think it is certainly important to provide richer detail of the past environments that humans in particular and hominids in general have adapted to. The Pleistocene certainly does not represent a homogenous ecological or social environment, and ancestral humans themselves have of course changed in important respects over the last five million years. The general point urged by evolutionary psychologists however - that adaptation is a historical concept - remains valid.

What is required then for complete explanations of human characteristics are detailed reconstructions of past environments and those organisms which have populated them. The evolutionary programme in psychology, therefore, needs to draw on the full resources of archaeology and paleoanthropology, which have begun to provide increasingly richer theoretical accounts of hominid behavioural, social and cognitive evolution and the ecological contexts in which it has occurred (e.g., Foley & Lee, 1989; Foley, 1987, 1996; Mithen, 1996; Mellars, 1989). It is also apparent that although environments have changed in important respects since the emergence of hominids, there are many features which have remained relatively stable. Studies on contemporary populations, therefore, are not just a means of evoking some vestigial mechanisms that have evolved to operate under radically different circumstances, but may also reflect the proper functioning of the systems concerned under normal conditions. This kind of analysis naturally has to be taken on a case by case basis and should be informed by those relevant studies in paleoanthropology. Part of the problem in detailing those aspects of the environment that humans have evolved to respond to hinges on the nature of the mind that has evolved to interface with the environment to produce adaptive behaviour. It is here, I would argue that human behavioural ecologists and evolutionary psychologists differ most in their approach.

Reconstructing human cognitive architecture: degrees of domain specificity

The approach of evolutionary psychologists favours a cognitive architecture that is richly endowed with a collection of domain-specific mechanisms which serve specific functions in response to typically a limited set of environmental conditions. Human behavioural ecologists, on the other hand, although not favouring an all-purpose domain-general mind, argue that there must be a considerable degree of domain generality to account for the flexibility of human behaviour. Alexander (1990), for example, has argued that several domain-general mechanisms such as consciousness and social imitation can provide the foundation for a wide range of adaptive behaviour across a variety of different circumstances.

There are several broad challenges to the idea of a domain-specific or modular cognitive architecture. Firstly, there is the problem of how a modular mind can account for the integration of information across a wide variety of different domains. An allied problem is how modularity can lead to the creativity and flexibility so apparent in human thought and behaviour. Thirdly, the full range of human abilities seems difficult to account for given a modular mind. How can an innately specified domains-specific mind possibly lead to the current abilities of humans in a wide range of specialities, such as advanced mathematics or musical composition? All of these problems seem to suggest the presence of more domain-general processes favoured by human behavioural ecologists such as Alexander (1990).

It is certainly true that humans can integrate information from a variety of different sources and are highly flexible in their behavioural responses. Such conceptual integration is at the heart of much of science and is embodied in our everyday use of analogy. It is just such cognitive creativity which led Fodor (1983) to construct his two-tier model of the mind: highly modular perceptual processes and non-modular conceptual ones. However, a domain-specific approach can handle this problem of conceptual integration and others in a variety of ways.

To begin with it is worth making the distinction between modules and domain-specificity. Modules can be said to refer to specific, neurally isolatable mechanisms which operate on specific classes of information. Domain specificity can simply refer to responses which are framed in terms of a specifically delineated domain. It follows that domain specificity can be underwritten at the neural level by brain circuits that serve a variety of functions, and may be spatially distributed in the brain. The notion of functional specialisation, as Shallice (1988) suggests, may be a more useful concept than strict modularity, certainly in terms of central processes. We can still posit domain specific mechanisms for something like jealousy, therefore, even though these mechanisms may draw on different neural circuitry, say those which realise anger, grief and fear. This suggests that domain-specificity does not entail strict informational encapsulation and that there may be considerable exchanges of information between functionally distinct subsystems. Indeed, among many who favour modular approaches to human cognition there tends to be an emphasis on the way different systems are integrated together, and the way these systems are influenced by a host of environmental factors. For example, Gardner's (1983) theory of multiple intelligences stresses the role that cultural factors play in the development of the different kinds of intellectual competencies. Karmiloff-Smith (1988) also takes a developmental perspective in examining the process of modularization which involves a series of phases that result in the redescription of implicit knowledge in a way which leads to more explicit representation. Following Karmiloff -Smith, Mithen (1996, 1997) has proposed a similar transition, but one which can be viewed on the broader time-scale of hominid evolution. Specifically, Mithen proposes that from a mind dominated by domain-general processes, early hominid minds became increasingly specialised with distinct, isolatable modules subserving language, natural history, technical and social intelligences. These isolated processes became increasingly integrated in the minds of anatomically modern humans and were responsible for the flowering of art and material culture in the upper Palaeolithic.

Another modular approach to the problem of cognitive creativity would be simply to suggest the evolution of an increasing number of domain specific processes (e.g., Cosmides & Tooby, 1994). Although conceptually messy, the gradual addition of many micromodules may well give the appearance of domain-generality in terms of the flexibility of responses that they allow. The view of evolution as a tinkering process (Jacob, 1977) and the idea that the mind is a kludge (Clark, 1989) - a cobbled together product selected for efficacy not elegancy - is congenial to this idea of profligate domain specific mechanisms.

Dan Sperber (1994, 1996) suggests a somewhat different solution to the flexibility problem. Sperber accounts for the human ability to integrate information from different contexts by positing another module: one that has the function of meta-representation.

This module naturally accumulates representations from a wide variety of different sources which are available for reflexive analysis. The domain of this meta-representational module is simply the set of all those representations which humans are capable of apprehending. Through language and the transmission of ideas, the content of this meta-representational module becomes bloated with all manner of beliefs and concepts. According to Sperber it is this module which underlies human creativity at the conceptual level, and allows us to entertain the esoteric ideas of quantum physics and the epic narratives embodied in mythology.

Accounting for novel behaviour and new, apparently maladaptive, traits is also possible from a domain specific perspective. Sperber's (1994) distinction between the *actual* and the *proper* domains of modules is usefully employed here. Although modules have evolved to respond to specific classes of input, there may well be novel features of the environment which also fulfil the input conditions of the module, and hence the domain of the module may be extended. In general outline, this is also the approach adopted by evolutionary psychologists such as Cosmides and Tooby (1994) to explain novel behaviour. Different inputs will lead to different outputs even though the underlying mechanisms may remain invariant.

It would be fair to say that there remain many unanswered questions regarding the extent and nature of modules or domain-specific mechanisms. However, on both conceptual and empirical grounds it is highly unlikely the mind is overwhelmingly domain general in its functioning, although there are surely some domain general mechanisms. Just how many domain specific mechanisms there are and how they should be identified remains somewhat of an open question. Certainly, much of the debate between human behavioural ecologists and evolutionary psychologists hinges on the way the cognitive architecture of the mind is ultimately characterised. At present there is no definitive answer to the way the mind is organised, although the extremes of adaptive flexibility, sometimes posited by human behavioural ecologists, is unlikely to reflect the specific evolutionary function of domain general processes. Such flexibility in behaviour may however be explicable in terms of some set of processes which do have proper functions under normal conditions.

Ignoring groups: Evolutionary psychology and the social environment.

Another line of criticism directed against the approach adopted by the evolutionary psychologists is to suggest that their perspective is overly individualistic in nature. Caporael and Brewer (1989, 1991), Brewer and Caporael, (1990), Caporael and Baron, (1997) have argued that evolutionary psychologists, by focusing on psychological mechanisms, have ignored the crucial role of social groups in hominid evolution. Although not advocating the role of group *selection* in human evolution, Caporael and Brewer (1991) argue that the social group has been an especially important selection *environment* for humans. *Social* psychology it is argued, rather than cognitive psychology, should be the focal discipline in evolutionary studies of human behaviour. The general point made by Caporael and Brewer is a fair one: humans have evolved in the context of social groups, so many of their cognitive and behavioural adaptations should reflect the complex demands of group living. However, although leaning somewhat towards methodological individualism, the approach of evolutionary psychologists has not been to ignore the role of the social environment in developing adaptation explanations of psychological phenomena. The work by Cosmides and Tooby (1989, 1992) on reciprocal altruism for example is explicitly developed in terms of psychological adaptations for the complex dynamics of reciprocal exchange, although admittedly the focus here is on dyadic interactions rather than larger group processes.

The social level of explanation in terms of emergent group properties and in terms of the social environment are clearly important aspects of the evolutionary programme in psychology. Any complete account of the evolution of human mind and behaviour must take notice of the importance of social factors. However, rather than viewing social psychology as the focal discipline in the evolutionary programme in psychology, I would suggest that it is better conceived as one important source of empirical and conceptual resources that can and should be drawn upon.

A somewhat different perspective on groups has been forwarded by David Sloan Wilson (1997). Rather than simply viewing groups as an important selection environment, Wilson argues that evolutionary accounts of human mind and behaviour must also consider the possibility of selection operating at the group level. Wilson argues that modern analyses of fitness tend to average fitness across groups therefore obscuring the possibility of inter group differences and the likelihood of group selection. An intriguing case study is presented by Wilson which suggests than human decision making may have evolved in part due to group selection processes. Although individuals are fully capable of autonomous decision making, groups in their decision making processes display properties not reducible to individuals, such as cognitive division of labour and group cohesiveness. Such differences, among others, would lead to differences in decision-making efficacy between groups, and hence be open to group selection.

There is considerable dispute in the philosophy of biology regarding the appropriate level of selection which evolution can be viewed as operating on. The standard Darwinian picture views evolution as a struggle between individuals culminating in differential replication. Although Darwin did admit the possibility of group selection, especially as an explanation for human morality. As Hull (1984 p. 142) expresses it "genes mutate, organisms are selected and species evolve" This standard view has been challenged by various alternatives operating at both below (Dawkins, 1976, 1982, 1996) and above (Wilson & Sober, 1994; Gould, 1989c; Eldredge, 1995) the level of the organism. Richard Dawkins has vigorously and eloquently argued that we should consider the gene as the unit of selection. Groups, species and even clades have also been considered as possible alternatives (Gould, 1989c; Brandon, 1990). A radically different approach argues that it is entire developmental systems which are inherited and which should be considered as the appropriate unit of selection (Griffiths, 1992b; Griffiths & Gray, 1994).

As there is no widely agreed upon consensus regarding this issue, the best strategy is to embrace the plurality of approaches and at least consider the possibility of group selection processes operating during the course of human evolution. The relative importance of group selection in human evolution, as in the rest of biology, however, awaits further empirical and conceptual developments.

Summary

Overall, the similarities between the various approaches to evolutionary explanations of human behaviour far outweigh the differences. First and foremost, all perspectives are clearly framed as part of the general evolutionary research programme and draw on the theory of natural selection and the various auxiliary hypotheses in developing their explanatory accounts. Most of the differences are also masked by a general desire to provide a unifying explanatory framework for psychological phenomena. As such, all perspective draw, wherever relevant, on comparative studies to further their explanations. The general idea is that one general explanatory framework should be sufficient to account for patterns of behaviour in both humans and other animals.

Part of the difference in various approaches can be attributed to institutional factors, which follow from the differences in focus between anthropologists, psychologists and sociologists. However, there are also real conceptual and methodological differences between the different perspectives which pivot primarily on different understandings of the notion of adaptation, and different views on the appropriate way to characterise the human mind. At our current state of knowledge the best overall strategy is to promote a plurality of different approaches and to evaluate them as progress in the relevant areas is achieved.

In chapter two I argued that psychology, like other sciences, should be aiming for complete explanatory accounts of the phenomena within its domain. These complete accounts are likely to include multiple explanations drawn over different temporal, conceptual, and spatial levels of organisation. The evolutionary programme in psychology should seek to develop such complete accounts. As Kitcher (1985, 1990) has argued, we need to provide rigorous and detailed accounts of the relevant genetic, developmental, physiological, psychological, historical, and functional factors which are important in any particular case. As such, the two approaches that I have discussed in this chapter - human behavioural ecology and evolutionary psychology - are potentially compatible with one another. Detailed cross-cultural accounts of patterns of behaviour under various ecological circumstances, elaborations of psychological mechanisms, and developmental and social information are all pertinent in explanations of most psychological phenomena.

The general message here for the evolutionary research programme in psychology is that it needs to draw on the full resources of the general evolutionary research programme and allied disciplines. To elucidate the nature of human cognitive architecture, for example, relevant information should be drawn from cognitive psychology, cognitive ethology, palaeoanthropology, cognitive archaeology, evolutionary biology, social psychology and neuropsychology. These sorts of syntheses are beginning to emerge as illustrated by the models of human cognitive evolution developed by a variety of authors (e.g., Mithen, 1996; Corballis, 1989; Donald, 1991, Dunbar, 1996). Evolutionary psychology and human behavioural ecology, therefore, should be considered as truly interdisciplinary in nature and attempt to provide complete explanations of psychological phenomena.

Chapter four

Challenges to the evolutionary programme in psychology: An overview

Evolutionary explanations of human mind and behavior have been forwarded since the origin of the evolutionary programme itself. Starting with the fact of natural selection and its applicability across all forms of life, it has seemed reasonable by many to explain those specific features of human nature that psychologists are interested in by reference to evolutionary theory. However, despite the long history of attempts to explain psychological phenomena from an evolutionary perspective, the evolutionary programme has become nothing like the dominant theoretical view in psychology.

One can postulate many reasons for evolutionary theory failing to obtain conceptual hegemony in psychology. Perhaps the evolutionary programme is simply theoretically inadequate. Although it may explain a diverse range of phenomena in other animals, it might be considered explanatorily impotent when it comes to explaining human psychological phenomena. Or perhaps instead, or as well as it's explanatory inadequacy, evolutionary explanations of psychological phenomena are seen as being morally unacceptable in that they are perceived as promulgating an unsavory and ultimately dangerous view of human nature.

I believe that it is fruitful to consider the challenges to the role of evolutionary explanations in psychology in terms of questions regarding the programme's overall explanatory coherence (Thagard, 1992). Moreover, the way that the evolutionary programme is related to issues of social and moral concern, has also been instrumental in restricting the scope of evolutionary theory in the human sciences. In general then, the viability of evolutionary explanations in psychology can be judged by assessing the global adequacy of the evolutionary research programme and its relations to the relevant domains of inquiry, other research programmes, and to society.

In the following four chapters I consider the various criticisms which have been directed at evolutionary explanations in psychology and the social sciences. Chapter five reviews the criticisms that have arisen as the result of the perceived moral inadequacy of the evolutionary research programme. Regardless of it's truth value[1], the evolutionary programme has been perceived by many to be forwarding a view of human nature which is likely to retard social progress and which can only serve to legitimize inequalities in society. Recalling the model of global research programmes outlined in chapter one, this challenge can be viewed as being directed at the nature of the relationship between the programme in general and society, and between the internal social features of the programme and the kinds of theories that have been developed. If the social features of the programme can be shown to have exerted an undue influence on theory construction and development, or the relations between the programme and society are inevitably morally pernicious, then the overall acceptability of the evolutionary programme in psychology is diminished.

Chapter six reviews criticisms of the evolutionary programme in psychology which have been directed at the use of comparative studies in furthering evolutionary explanations of psychological phenomena. These criticisms can be conceptualized as challenges to the use of analogical reasoning in the evolutionary programme and in terms of questions regarding the explanatory breadth of the programme. As outlined in chapter one, explanatory breadth and analogy are important components of Thagard's model of explanatory coherence. If it can be shown that the use of analogical reasoning in general is invalid and that evolutionary explanations cannot be extended from non-human to human animals, then the overall explanatory coherence of the programme would be decreased.

In chapter seven I discuss the role of adaptation explanations in psychology. The challenges to the evolutionary programme are various here. Firstly, and most importantly, it is suggested by some that most of the phenomena that psychologists study cannot be reasonably characterized as evolutionary adaptations, nor can they adequately be explained by reference to adaptations. The challenge, in this case, is to the explanatory breadth of the programme. If the evolutionary programme only has

[1] Although ethically based critiques of evolutionary explanations often link the claim of moral inadequacy with scientific inadequacy.

the conceptual resources to explain a small subset of psychological phenomena, then the role of evolutionary explanations in psychology is likely to be considerably restricted. Evolutionary psychologists and sociobiologists have also been criticized for their use of what are termed 'just-so stories': claims for adaptations based on limited evidence. The challenge to the evolutionary programme is two-fold here. Firstly, there is a challenge to the empirical adequacy of the programme. That is, it is suggested that adaptation explanations are developed on the basis of little empirical evidence. Secondly, there is the suggestion that there is a needless proliferation of explanatory hypotheses which are deployed to explain any anomalies to the evolutionary programme which may occur. As the coherence of an explanatory system is decreased relative to the number of propositions that it contains, this criticism can be seen as an another challenge to the overall explanatory coherence of the evolutionary programme. Finally, it is suggested that there are perfectly adequate alternative explanations for psychological phenomena that do not need to invoke the concept of adaptation. Competition between theories is, of course, an important component of theory appraisal. If it can be shown that there are better alternative explanations for various psychological phenomena then those offered by the evolutionary programme, then the acceptability of evolutionary explanations in psychology will be diminished.

One important source of competing explanations for psychological phenomena is to suggest that they can be explained as a consequence of culture, or more specifically, social learning. If the role of social and cultural factors in the explanation of psychological phenomena is viewed as central or primary, then it is suggested that the scope of evolutionary explanations, and hence their overall explanatory coherence, may be considerably attenuated. In chapter eight I discuss the various criticisms directed at evolutionary explanations in psychology from this perspective.

There are several general points that should be noted in my discussion of the various issues outlined above over the next four chapters. Firstly, the various criticisms need to be taken on a case by case basis. That is, it may be that the criticisms hold for some specific explanations, or instances of explanation, but not for others. However, given this point, we can also evaluate the criticisms as they hold generally for the evolutionary research programme in psychology. In this context it is worth considering whether the various criticisms can be met by improvements in the appropriate portions of the programme or whether they *necessarily* hold for evolutionary explanations in general.

Secondly, as theory appraisal is always a comparative affair, ideally the explanatory coherence of the evolutionary programme should be directly compared to alternative theoretical accounts. However, although I do discuss alternative sources of explanation, most of the criticisms of evolutionary explanations in psychology do not typically offer coherent and integrated alternative theoretical accounts. Instead, the comparative process seems to be done on a piecemeal basis, if at all. Moreover, it is often the case that alternative theoretical accounts are perfectly compatible with evolutionary ones. The issue of the compatibility of evolutionary explanations with alternative theories is addressed in more detail in chapters nine and ten.

In general I shall provide reasons to believe that the explanatory coherence of the evolutionary programme is not considerably diminished by the various sorts of criticism that have been leveled against it. What the replies to the various criticisms do suggest, however, is the kind of role that evolutionary explanations are likely to play in psychology. The precise nature of this role will be discussed in more detail in the final chapter.

Chapter five

Ethical issues in the evolutionary research programme in psychology

It has become increasingly clear in recent years that science is permeated with values at all stages of the scientific process. These values can be crudely, but helpfully, delineated as either epistemic or non-epistemic in nature. As I have discussed in Chapter One, a host of epistemic values such as explanatory breadth, simplicity, and empirical adequacy, are employed in evaluating the worth of theories and in adjudicating between alternative research programmes. Values of a non-epistemic nature, such as, for example, those embodied in political ideology, can also influence the course of scientific inquiry. As a realist I have rejected the idea, advanced by some, that such non-epistemic values typically *determine* the nature of scientific inquiry. However, as a naturalist I believe that it is also clear that science is, importantly, a human endeavor, and that the ideological world views of scientists and the cultural milieu in which they are embedded can potentially influence the nature of the scientific process. Moreover, the results of science embodied in empirical research and articulated in theory can have a profound influence on important issues of general moral concern for society. The idea that science can be insulated from such effects is neither viable nor ethically acceptable. It follows that the acceptability of a theory or research programme is not only influenced by the kinds of values embodied in Thagard's theory of explanatory coherence, but also by a host or more or less explicitly moral values.

There are two related issues of concern here when evaluating the moral acceptability of a theory or research programme. One, have the theories, methodologies and empirical results been influenced to a notable degree by factors of a non-epistemic nature such as the class, race or sex of the scientists concerned, or by specific features of the institutions involved? And two, are the results of the scientific research such that they may impact in a potentially deleterious way on any entities of moral concern? These two different ways non-epistemic values can permeate science I will label internal and external values accordingly. This internal/external division can be elaborated further. Internal values can permeate science in a variety of ways. The choice of topic, the kinds of models and metaphors employed and the way empirical findings are conceptualized can all potentially be influenced by social and political factors. Although typically this kind of bias is both subtle and 'accidental' in nature, in extreme cases it may lead to fraud and fabrication, as in the Cyril Burt affair (Joynson, 1989). In evaluating theories we should only be concerned if this kind of bias provides us with a significantly distorted view of the world. External values also can play an important role in science. The social and political impact of a theory may seem to be isolated from the actual process of theory development and appraisal. However, although the suppression of research should only be carried out in extreme circumstances, it is reasonable to expect greater standards of evidence, and more coherent theories in areas which have a greater capacity for social impact. Similarly, questions of risk assessment should enter the evaluative process, and be one means whereby theories are deemed as more or less acceptable. The model of global research programmes that I outlined in chapter one incorporates these valuational aspects as important parts of the overall structure of a research programme itself. It follows that in any global assessment of a research programme's adequacy the role of both internal and external values will need to be elucidated.

The way science can be impregnated with values is clearly seen in the history of the evolutionary research programme and the various ways in which evolutionary ideas have given birth to certain social policies and beliefs in society. The key figures in the development of evolutionary theory in the 19th century were, without a doubt, products of their time and widespread racist and sexist ideas penetrated the biological sciences in a variety of ways at this time. For example, the attitude of Europeans towards other races clearly played a role in the widespread belief that different races were the product of evolutionary divergence in our distant past. Different races were conceived by many as distinct sub-species which explained the 'manifest differences' in their intellectual and social abilities. This kind of bias in evolutionary anthropology probably reached its apotheosis in the explicit fraud of Piltdown man, the acceptability of which was dictated in part by its conformity with notions of European superiority (Gould, 1981). There are two cases here where the development of theory and the interpretation of evidence can be seen to be influenced to some extent by pre-existing social and political biases.

The social *impact* of evolutionary theory is perhaps most evident in its use, by some, to justify a host of morally dubious social practices and to legitimise inequalities in society. Of greatest notoriety must be the explicit 'use' of evolutionary theory to sanction selective immigration and sterilization policies in America and elsewhere, culminating ultimately in the racial 'cleansing' of Nazi policy. This misappropriation of Darwinian theory to validate overt selective regimes continues in some areas, as illustrated in the recent Singaporean policy of providing incentives for the more educated to breed and the less educated to suppress reproduction (Gould, 1985).

The acceptability of the evolutionary research programme in psychology and the social sciences has also been challenged because of explicit claims that it embodies and justifies a range of values which many see as socially pernicious. The extent of feeling that many experienced with the development of sociobiology is demonstrated by the extraordinary reaction to E. O. Wilson and his sociobiological views. Following the publication of *Sociobiology* there rapidly appeared a strong left-wing reaction, leading to the formation of the group Science for the People, explicitly set up to challenge the ideas of sociobiology. Various forms of protest also occurred, including leaflet campaigns, the shouting of anti-sociobiological slogans during Wilson's lectures, and the dousing of Wilson with cold water by protesters at a lecture (Wilson, 1994).

The general charge against sociobiology and allied attempts to explain human phenomena from an evolutionary perspective is that such approaches promulgate a determinist view of human nature which serves to legitimize the social and political status quo (e.g., Allen, 1978; Sociobiology Study Group, 1978; Sahlins, 1976; Rose, Kamin, & Lewontin, 1984). There are three interrelated criticisms of the evolutionary programme in psychology here: (1) That it demonstrates bias explicable in terms of the race, class, and sex of the scientists concerned; (2) that it espouses a fallacious genetic determinism; which (3) justifies current social arrangements by claiming that they are both natural and inevitable. These general arguments are summed up by Rose et al. (1984, p. 236): "Sociobiology is a reductionist, biological determinist explanation of human existence" whose appeal "is in its legitimization of the status quo. If present social arrangements are the ineluctable consequence of the human genotype then nothing of any significance can be changed." Sociobiology is charged here with committing the naturalistic fallacy. As Allen (1978, p. 261) explains: ". . . for Wilson, what exists is adaptive, what is adaptive is good, therefore what exists is good"

Sociobiology is viewed as part of a long history of attempts to explain human social behaviour from an evolutionary perspective in a way which justifies inequalities in society. "There is nothing that separates the programme and specific claims of the social Darwinism of the 1870's from the Darwinian sociobiology of the 1970's" (Rose et al., 1984, p. 243). I think it is important to separate two lines of criticism in the accounts presented by Rose et al. (1984) and others. In brief, the first criticism argues that evolutionary explanations of human behaviour represent *bad* (or at least misguided) science; the second line of criticism argues that it is *dangerous* science. The next three chapters is devoted to a discussion of the epistemic worth of the evolutionary programme in psychology, so I will confine myself here to a discussion of the moral acceptability of the programme. Once this is done, I think it becomes clear that the main force of the ideological attack on the evolutionary programme in psychology is absorbed by a better and more sophisticated understanding of how evolutionary theory is, and can be, used to explain human behavior. There still remains, however, a significant residue of criticism which does raise important questions regarding the role of non-epistemic values in the evolutionary programme. I shall discuss these various ethical challenges to the evolutionary programme in psychology in terms of claims of class, sex, and race bias.

Class

Vigorous attacks have been made on biology in general and sociobiology in particular on the basis that they embody a pervasive and pernicious ideological bias (e.g., Sahlins, 1976; Rose et al., 1984). "What is inscribed in the theory of sociobiology is the entrenched ideology of western society: the assurance of its naturalness, and claim of its inevitability" (Sahlins, 1976 p. 101). I shall discuss this charge in terms of both internal and external values. In formulating evolutionary theory and in particular the theories of sociobiology, it is argued that there is an entrenched class bias. The strong claim here is that biological determinism with respect to political class was *invented in order* to further certain political aims. "It is precisely to meet the need for self-justification and to prevent social disorder that the ideology of biological determinism has been developed" (Rose et al., 1984 p. 68). A somewhat weaker, but still important criticism, is that the terminology, concepts, and theories of the evolutionary programme have been borrowed from capitalist ideology and applied, inappropriately, to the natural world. Metaphors such as competition, investment, niche, division of labour, cost/benefit analysis, and so forth, have been appropriated from capitalism, it is argued, and been transferred to the biological realm (Sahlins, 1976). As such, it is suggested that our Western view of nature is permeated by the notions of individualism and exploitation. Sahlins (1976) in particular, sees a pernicious slide from the notion of differential reproduction through competition between organisms, to self-maximisation and exploitation of others. Evolutionary theory and sociobiology are characterised as the intellectual culmination of Hobbes's *bellum omnium contra omnes*: the war of all against all.

What are we to make of these charges? Firstly, I think that the strong claim that the concepts of the evolutionary programme were formulated for the *explicit* purpose of justifying the social and economic stratification of western society can be rejected. Certainly there is no evidence that evolutionary thinkers from Darwin to Wilson developed their theories *as* political strategies. Indeed part of the reason that Darwin held off from publishing his theory of natural selection for some twenty years was his fear that it might be employed to further certain political ends (Desmond & Moore, 1991). The weaker claim, however, needs some consideration. Without doubt the choice of language, the use of metaphor, and the articulation of models must reflect to some extent the social and political climate in which they are developed. This point is almost trivially true. In searching for appropriate analogies and metaphors scientists can only draw on the corpus of their own beliefs, which are likely to reflect, in part, the historical and social circumstances of their existence. As long as we reject extreme forms of social constructionism, there is no necessary *bias* in this. Bias, however, can occur if these metaphors and analogies do not adequately represent those real processes operating in the world. That is, if they are retained and used *in spite* of what our interactions with the world tell us.[1]

I think there is some reason to believe that this might be the case in the development of evolutionary theory, although its effect on the growth of biological ideas has been relatively negligible, and falls far short of the pervasive bias indicated by Sahlins (1976) and Rose et al. (1984). Darwin himself, influenced by the ideas of both Malthus and Adam Smith, made extensive use of the notion of competition in the struggle for finite resources. Although Darwin made explicit note of the metaphorical nature of this struggle (for example, it could be said to refer just as much to a plant 'struggling' with the inclement conditions at the edge of a desert as two lions snarling over a wildebeest carcass), it is certainly true that the focus was on individual competition. In contrast, the Russian biologist Kropotikin in *Mutual Aid* paid more attention to the important role of co-operation in nature. This emphasis may reflect both the Russian political and ecological environment (Gould, 1991d). However, Darwin did not *ignore* the role of co-operation in evolution, and indeed both co-operation and exploitation are rife throughout the natural world. Indeed, as I have argued in the previous chapter, the evolutionary programme in psychology needs to, and to some extent has, emphasised the importance of both individual conflict and group living in developing its theoretical structures. Even E. O. Wilson, who tended to favor reductionist style explanations, explicitly developed an explanation of human morality in terms of group selection processes and adaptations for group living, as of course did Darwin. From a realist perspective, the explanatory success of the evolutionary research programme in general, suggests that the metaphors and concepts employed tell us something about the real world, and do not *merely* reflect a blanket ideological bias.

As well as charging the evolutionary programme with internal ideological bias it is also claimed that evolutionary theory is employed in order to legitimize the *status quo* in society (Sahlins, 1976; Rose et al., 1984). It is suggested that this justification for

[1] Of course it might be argued here that our beliefs and desires influence the way that we perceive the world so that we cannot accurately determine whether the concepts that we use are true representations of reality. Although I think this claim must be true to some extent, as I have argued in chapter one, the history of science suggests that the concepts that people possess do not *determine* their percepts, and hence evaluations of the truth of theories is possible.

present inequalities is seen most acutely in evolutionary explanations of social behaviour ". . . sociobiology contributes primarily to the final translation of natural selection into social exploitation" (Sahlins, 1976 p. 73). There is no doubt here that evolutionary theory has been pressed into service as a scientific means to buttress the ideological beliefs of many. That arch capitalist, John D. Rockefeller, for example, was reported to have stated: "The growth of a large business is merely a survival of the fittest . . . this is not an evil tendency in business. It is merely the working out of a law of nature" (quoted in Rose et al., 1984 p. 26). Darwinism, it was considered, could provide a legitimisation of the laissez- faire politics of capitalism by placing the process squarely in the natural realm. If such a process was natural, it was seen by many to be good.

These ideas, often labeled Social Darwinism, are typically traced to the 19th century evolutionary thinker Herbert Spencer. Spencer believed that there were inexorable laws of nature that governed the physical, mental, and social realms. Human nature moves towards perfection by its complete adaptation to the social state. It followed in Spencer's philosophy that government interference can only deform this natural progression. The enactment of poor laws and the like were seen by Spencer as retarding the progress of the species (Richards, 1987). Left-wing critiques of evolutionary biology argue that these ideas merely illustrate the naturalistic fallacy: it is not possible to derive *is* statements from *ought* statements. Just because some state of the world has evolved it does not follow, therefore, that it is morally good (Rose et al., 1984). Richards (1987) for one, however, has exonerated Spencer from the charge of committing the naturalistic fallacy. Spencer's moral philosophy was based on the oft invoked ethical maxim of the greatest happiness principle. This principle was not derived from the fact of evolution, but rather evolution was seen as the means to realise this goal. The dubious implications of Spencer's philosophy, therefore, can be seen as a consequence of fallacious ideas about evolution and society rather than an outright violation of deductive logic and the direct transfer of is to ought.

Capitalists such as Rockefeller and those advocating extreme forms of eugenic policy can be less easily pardoned. The belief that a forced reduction in the reproductive output of the lower classes was morally justified by an invocation of Darwin's theory of natural selection is clearly an example of faulty reasoning. Indeed any *overt* intervention in human society can hardly be seen as simply an enactment of *natural* selection.

Evolutionary theory certainly can, and has been, put to malfeasant ends, but is this typically the case with the current evolutionary programme in psychology? It is important to note that the critique presented by Rose et a*l*. (1984) hinges on a strongly genetic determinist reading of evolutionary explanations of social behaviour. It is not only the claim that certain social arrangements are the product of evolution that incites moral concern, but the claim that these arrangements are genetically determined and therefore inevitable. For Rose et al. (1984, p.18) biological determinism is clearly politics by another name.

> For if human social organization, including the inequalities of status, wealth, and power, are a direct consequence of biology, then, except for some gigantic program of genetic engineering, no practice can make a significant alternation of social structure or the position of individuals or groups within it. What we are is natural and therefore fixed.

If this really *is* the belief of evolutionary social scientists then there is certainly cause for concern and a clear case where the social impact of a scientific theory counsels extreme caution in its development and promulgation. I will defer a more detailed discussion of determinism to later in this chapter, but I note in passing that I can seen no evidence that any evolutionary biologists or psychologists subscribe to the view of genetic determinism which is sometimes attributed to them. Even Wilson (1975, 1978), who leans most closely to the genetic determinist perspective, clearly invokes the role of the environment in influencing final outcomes. However, Wilson does see social and cultural evolution as *constrained* by biological processes. ". . . there is a limit, perhaps closer to the practices of contemporary society than we have the wit to grasp, beyond which biological evolution will begin to pull cultural evolution back to itself" (Wilson, 1978, p. 80). Wilson is really making a claim here about the norms of reaction of human behaviour (Kitcher, 1985). There is only a potentially narrow set of possibilities in which human societies can be arranged. "Biological constraints exist that define zones of improbable or forbidden entry" (Wilson, 1978 p. 81). It is understandable how these sorts of claims can elicit genetic determinist interpretations.

However, I think it is clear that even for Wilson current social arrangements are not some *ineluctable* consequence of evolutionary processes.

Indeed, a more thoughtful evolutionary analysis would suggest that the present social stratification in Western societies is an anomaly in evolutionary terms. For the vast majority of our evolution hominids are likely to have subsisted in small groups of a generally egalitarian nature. Although it is highly likely that these societies would have exercised some form of division of labour, this would have been primarily on sex rather than class lines. The accumulation of wealth and material possessions was possible only since the relatively recent advent of agriculture. The class based society found in many contemporary populations, therefore, can not be justified by reference to evolutionary theory even if the means of justification were legitimate.

The current evolutionary programme in psychology and the social sciences, therefore, can not be deemed as unacceptable due to any pervasive class bias. There is a need, however, to understand human psychological phenomena as a product of both competition and co-operation, and not to subscribe to overly individualistic models of human evolution. This need, I would suggest, follows from the likely process of evolution rather than any specific ideological bias. Given the often considerable slack between academic formulation, popular reporting and the understanding of the public, there is also a constant concern for precise formulation and constant qualification of evolutionary explanations of human behaviour. This is true of ideas regarding social stratification, but perhaps more so of those pertaining to sex and race.

Sexism and evolutionary theory

The evolutionary research programme in psychology has also been charged with a pervasive and pernicious sexism. The criticism is summed up in a recent paper by Travis and Yeager (1991, p. 127).

> The persistence of and tolerance for the theoretical and empirical limitations of sociobiological applications to human behaviour suggest that human sociobiology serves a pervasive sexism in modern society. While much of science may be socially constructed, it is also the case that science is often

used as a camouflage for bias in the construction of social life. Bolstered by reductionism and biological determinism, a narrow explanation of very complex social patterns has been socially constructed in a way that reflects and supports historical and cultural values more relevant to a sexual status quo than to the scientific investigation of evolutionary theory.

This sexist charge, like the one made against class, has both an internal and external dimension. The internal claim for sexism is predicated on the idea that the language, concepts, and models of the evolutionary programme reflect an androcentric bias. Moreover, it is argued that this bias has exercised an influence on the way the world has been perceived. The primatologist Sarah Hrdy (1986) for example, has documented how research in primatology has been male focused. Until very recently the male orientation of primate research, Hrdy argues, has resulted in the fostering of the myth of coy females and ardent males, and diverted attention way from important areas of research such as male paternal care and female-based dominance systems. More recent research indicates that females, far from being coy, are often seen to solicit copulations from males. The role of males in offspring provisioning in a number of primate species has also been revised. The point Hrdy makes is that the *preconceptions* of male primatologists has led to a representation of primate behaviour not wholly consistent with the truths of primate society, as they are currently being revealed.

I think there is something to this charge, especially when the results of primate research have been used to explain patterns of sexual differences found in human society. It is likely the values of male scientists have influenced to some extent the course of research relating to sex differences in behaviour. These values themselves are perhaps explicable (although not excusable) in the light of evolutionary theory. Human males due to a chronic uncertainty over paternity should be more concerned with their partner's sexual infidelity than females. To the extent that females benefit from extra-pair copulations - and recent research suggests that this benefit may be considerable (Baker & Bellis, 1995) - then they should be more concerned with concealing sexual infidelity and males may be more motivated to believe in female coyness.

A full expose of these kinds of biases that have influenced research on the evolutionary origins of sexual behaviour can only serve to further progress in this field, and will not by themselves invalidate the nature of the programme itself. The *recognition* of these kinds of biases serves as a rejection of the kind of constructionist philosophy of science promulgated by Travis and Yeager (1991). The way we view the world is surely influenced by a host of factors relating to sex, class, and so forth, but it is not *determined* by these things. Moreover, the kinds of sex differences, which have been suggested by various researchers, are typically consistent with evolutionary theory and are not usually formulated *ad hoc* to shore up a male dominant political system.

The stronger claim made by Travis and Yeager and others is that the evolutionary research programme has been used to justify sexual differences in society as both natural and inevitable. The view of females as naturally coy, submissive, passive homemakers and males as naturally dominant, powerful breadwinners is certainly one that is bandied about in the public sphere from time to time. This suggests that evolutionary scientists need to be cautious in their research on sex differences especially in areas that have potential political impact. However, research directed at identifying and elucidating sex differences is not implicitly sexist in nature, nor do evolutionary scientists typically claim that current differences between men and women in society reflect *inevitable* patterns of differentiation.

The tremendous volume of research on sex differences in mind and behaviour in psychology seems to indicate that there are real differences between men and women and that these differences are not wholly explicable in terms of different patterns of socialization. For example, there is a consistently reported difference in the spatial abilities of men and women. Men typically are superior to women on a range of spatial tasks. This difference has been explained in terms of the male/female division of labour and the larger home range size of men in our evolutionary past (Gaulin & Hoffman, 1988). Silverman and Eals (1992) also argue, however, that women are superior at other spatial tasks, ones that involve the ability to recognise and recall the spatial configuration of objects. It is argued that this difference reflects the different selection pressures for women related to foraging. These differences in spatial ability between men and women, furthermore, seem to be associated with hormonal changes and do not reliably emerge until after puberty.

What are the social and political implications of these kinds of differences? Importantly, I think it is worth noting that social inequality and equality of opportunity are *predicated* on an understanding of underlying differences. If there are real differences between males and females then similar treatment will not elicit similar results. It follows that in order to achieve anything like equality we should be aware of any underlying differences between males and females and their putative causes, and develop our social policies accordingly (Tavris, 1992). Moreover, an elucidation of differences says nothing necessarily about the relative superiority or inferiority of those differences (Ruse, 1981).

It should also be clear that elucidating the evolutionary underpinnings of behaviour is not tantamount to justifying that behaviour. For example, even if rape is a facultative adaptation as Thornhill and Thornhill (1990) claim, this cannot in any way *legitimize* rape or render it morally excusable. These sorts of evolutionary claims however should be treated with caution. Given the potential political ramifications of claims like those made by the Thornhills, I would suggest that there should be greater standards of evidence for such hypotheses and due care in their presentation in the public realm.

Once overt claims about the genetically determined nature of evolutionary arguments are rejected I think the evolutionary programme in psychology can be freed from the charge of pervasive and pernicious sexism. However, we should always be aware of potential bias in research and investigations of a politically volatile nature need to be undertaken with due care.

Evolutionary theory and racism

The role of bias in the scientific investigation of race has a long and perfidious history. As Gould (1981) documents, both pre and post Darwinian evolutionary scientists, influenced by *a priori* notions of white supremacy, developed theories, manipulated data and presented results in a manner which served to reinforce their existing beliefs. The fashionable 19th century discipline of craniometry, for example, was utilised to demonstrate the supposed scientific basis for intellectual differences between different races. Indeed, many theorists viewed blacks and other races as intermediate species or sub-species between whites and apes (Gould, 1981). Needless to say the widespread prevalence of such views served only to legitimize racial inequality and to preserve existing social hierarchies.

Are such biases and beliefs prevalent in contemporary evolutionary accounts of human behaviour? Certainly Gould (1981) and others (e.g., Rose et al., 1984) think this is the case. Gould suggests that the hereditarian program in IQ testing is a simple continuation of spurious attempts to delineate races on the basis of supposed differences in mental ability. Furthermore, it is argued (Gould, 1981; Rose et al., 1984) that such attempts only serve to justify the status quo as both natural and unchangeable.

It is certainly clear that there *are* differences between different races in their IQ test scores. Just what these differences tell us and what their underlying cause or causes are is a matter of considerable debate. I do not propose to delve into this issue in any detail here, but I would say that at present it is unclear to what extent differences can be attributed to genetic variation or to environmental factors, if indeed we can make a conceptually sound distinction between these causal influences. However, regardless of the basis of IQ differences, it is highly unlikely that any cogent evolutionary account could be provided to explain the pattern of differences found. Given the likely recent origin of different racial groups, and the considerable interbreeding between different races that has occurred, it is implausible to suggest that there are *substantial* genetic differences in cognitive abilities between different races. Genetic differences between different racial groups is small, with within group variation exceeding between group variation (Tooby & Cosmides, 1990a). That there are *some* genetic differences between groups is clear, given differences in skin colour, physical form and the prevalence of certain genetic disorders. These differences may reflect some adaptation to local conditions, but are probably to a large extent due to chance factors such as genetic drift, and pathogen-driven variability at the molecular level. Given that psychological adaptations are probably underwritten by numerous genes and instantiated in complex developmental pathways, it is highly unlikely that different racial groups will demonstrate any significant innate differences in their cognitive abilities.

That is, of course, not to deny the possibility of small differences due to genetic factors. Regardless, the data analyzed by Flynn (1987) demonstrating the large intergenerational gains in IQ scores indicate that IQ scores themselves are unlikely to reflect any innate and immutable quality as is often assumed in the hereditarian programme in IQ testing. Furthermore, evidence for genetic differences in the intellectual ability between races by itself, as even those in favour of the hereditarian programme admit (e.g., Herrnstein & Murray, 1994), have no pernicious implications for social policy.

A slightly different criticism of evolutionary accounts (e.g., Rose et al., 1984), directed at Wilson and others, calls into question the idea that racism itself may have an evolutionary explanation. Wilson (1975, 1978) claims that xenophobia may have a genetic basis and reflect an evolutionary history where aggression between different groups may have been prevalent. The study of in-group/out-group biases, and attitudes towards different races, however, is surely a legitimate area of scientific inquiry, and one in which plausible evolutionary accounts can be developed. Although, as in research on sex differences, the way this research is conducted and the information disseminated needs to be carefully monitored, any further understanding we can glean regarding the sources of group conflict can only be helpful. That is, it is unlikely that we can make any substantial progress in ameliorating racial conflict without a more detailed understanding of the causal mechanisms that are responsible for its occurrence.

Summary: the role of non-epistemic values and the acceptability of the evolutionary research programme.

The claim that the evolutionary programme in psychology is permeated by values of a non-epistemic nature is predicated on three main arguments:

(1) The understanding of evolutionary processes has been distorted due to the class, race, and sex of the scientists concerned. At its extreme, this line of reasoning claims that evolutionary theory itself is a social construction which merely, or at

best predominately, reflects those interests of a white, male orientated, capitalist society.

(2) Evolutionary explanations of psychological and social phenomena are presented in terms of the deterministic outcome of genes. Biology is equated with destiny.

(3) There has been widespread and inappropriate use of the argument that because something has evolved it is therefore *good*. This leads to the justification of present inequalities in society as both natural, and in combination with point (2), as inevitable.

I believe that a fair assessment of these criticisms should be taken on a case by case basis. My objective in this summary, however, is to consider the general style of argument typically employed by sociobiologists and evolutionary psychologists while acknowledging a considerable diversity of ideas that have emerged from those wishing to explain human behaviour from an evolutionary perspective.

It is certainly true that cases of bias and distortion of theory and data have been present in many areas in the evolutionary programme (and, of course, elsewhere in science). Furthermore, these biases can, and have, had a considerable and at times chilling impact on social policy and public attitudes to questions of race, class, and sex. To further the aims of science, as I have outlined them in chapter one, these sources of bias should be identified and eliminated wherever possible. However, evolutionary theory itself is not *merely* a projection of the social beliefs and biases of certain classes of individuals. The understanding of evolutionary theory by contemporary scientists provides no basis for the existence of significant and overwhelming differences between different individuals. As Gould (1985) expresses it, human equality is a contingent fact of history. Indeed, evolutionary psychologists more often than not are keen to emphasise the unity of human kind as a consequence of species wide selection for universal psychological adaptations (e.g., Tooby & Cosmides, 1990a). Evolutionary explanations for race and especially class differences, therefore, are likely to be on shaky grounds. It is more plausible, from an evolutionary perspective, to postulate sex differences in behaviour; however, the presence of differences says nothing about the relative *value* of those differences.

A strong value based critique of the evolutionary research programme in psychology can, I believe, only be sustained if determinist legitimizations of inequality are widely promulgated. However, I see no evidence that this is typically the case, especially in recent discussions. Indeed, evolutionary scientists are usually at pains to distance themselves from any claims regarding the genetic determination of psychological traits and social systems. As Tooby and Cosmides (1992, p. 39) express it:

> Neither "biology", "evolution", "society" or "the environment" directly imposes behavioral outcomes, without an immensely long and intricate intervening chain of causation involving interactions with an entire configuration of other causal elements. Each link of such a chain offers a possible point of intervention to change the final outcome.

There is no question, therefore, that certain social arrangements are in any sense inevitable, as the history of human societies has demonstrated:

> . . . the human psychological system is immensely flexible as to outcome: everything that every individual has ever done in all of human history and prehistory establishes the minimum boundary of the possible. The maximum, if any, is completely unknown" (Tooby & Cosmides, 1992, p. 40)

Similarly, it is hard to see any evidence for the naturalistic fallacy in any contemporary evolutionary accounts in psychology. Although there is interest in the evolutionary antecedents of a wide range of human behaviour, there is no general claim that these explanations in any sense legitimise such behaviours. However, in developing appropriate ethical theories and formulating social policies we need to be aware of what humans are *capable* of. Is does not imply ought; but ought does imply *can*. This I think, is the general thrust of Wilson's position regarding the evolutionary limitations on social change. It follows as a more general point that in taking ameliorative action in the social sphere we need to have some understanding of the causal processes which underlie the current state of affairs and how they can be tweaked to realise our social goals. To the extent that evolutionary theory provides an adequate means for developing explanations in the social sciences, then it will have some role to play in the development of acceptable moral systems and in contributing to ethically motivated social intervention. In order to develop a fairer society and achieve what is of most value in existence we need to draw on the full resources of the evolutionary research programme as part of a general strategy to understand the causal underpinnings of human nature. As Tooby and Cosmides (1992, p. 40) argue " Knowing the details of the mechanisms involved will prove crucial in taking any kind of constructive or ameliorating action. 'Solutions' which ignore causation can solve nothing"

However, despite this exoneration of evolutionary explanations in psychology from charges of ideological bias, I believe it is important that scientists consider in some detail the likely social impact of their ideas, before they become widely distributed. As Kitcher (1985) points out, evolutionary social scientists need to proceed with caution because their theories have political clout quite apart from the political opinions of their supporters. As such, the quality of research and the interface between science, society and social policy need to be carefully monitored. I think it is fair to say that this has not always been the case and it is one area of the evolutionary programme in psychology to which due attention needs to be paid.

Chapter six

Anthropomorphism and analogy: the role of comparative psychology in the evolutionary programme

Evolutionary explanations of human mind and behaviour have often drawn heavily on comparisons with other animals. Certainly the 19th century evolutionary research programme in psychology made extensive use of comparative explanations to develop theories about the evolutionary origins of human psychological phenomena (e.g., Darwin, 1871, 1872; Romanes, 1881). In contrast, much of contemporary psychology has proceeded as though there were a sharp division between valid and valuable explanations of human mind and behaviour and that of all other animals. As such, comparative psychology has often been marginalised and seen simply as a source for animal models of human states and processes. In ignoring comparative psychology, Demarest (1987, p. 147) argues that psychologists ". . . have overlooked the one common thread that ties together almost all of the diversity found in mainstream psychology: an evolutionary perspective."

Comparative psychology plays an important role in developing evolutionary explanations in psychology in four related ways:

(1) Comparative psychology is utilised to elucidate adaptation via the study of homologous traits.

(2) Arguments from analogy are deployed as heuristic devices to further the claims for the evolutionary basis of similar psychological and behavioural traits.

(3) Comparative analyses can be used to establish general trends which occur across species.

(4) Comparisons in the characteristics between species are used in reconstructions of the phylogeny of human psychological and behavioural traits.

In general, comparative psychology plays an important role in assessments of the explanatory coherence of the evolutionary research programme in psychology. More specifically, the extension of evolutionary theory to psychological phenomena is seen as more plausible given cogent accounts of the same or similar phenomena in other species. Comparative arguments are used, therefore, in extending the explanatory breadth of the evolutionary programme. This extension is often achieved via analogous reasoning. If trait S has evolved in species T for the function of U, then an analogous trait S^1 in humans may also have an evolutionary explanation in humans in terms of function U. Even stronger claims for evolutionary explanations of human phenomena can be achieved for homologous traits, that is, ones which humans share with closely related species and which reflect lines of common descent.

Furthermore, advocates of evolutionary approaches to human behaviour point to the *unifying* power of the evolutionary research programme. If one *kind* of explanation can account for both human and non-human animal behaviour, then we should prefer this kind of explanation over separate explanatory accounts for humans and for other animals. Humans, it is argued, should be explicable ultimately in the same way as other animals.

Critics of evolutionary explanations in psychology, however, have questioned the role that comparative studies play in the development of explanations of human mind and behaviour. These criticisms can be viewed as challenges to the explanatory breadth of the evolutionary programme, and to the validity of analogous reasoning in developing evolutionary accounts of human characteristics. These concerns, it is argued, are exacerbated by a pervasive and fallacious anthropomorphism which sees the misapplication of human concepts to animal behaviour.

Challenges to the role of comparative psychology in the evolutionary programme

The firmest footing for comparative accounts of different traits should be found in homology. If two closely related species have a given characteristic not possessed by more distantly related species then there is *prima facie* evidence of a common evolutionary origin, genetic basis, and probably, adaptive function. However, many critics of the evolutionary programme argue that even arguments for homologous traits become mired in confounding factors when used to develop explanatory accounts of human phenomena. Lewontin (1990) argues that there is simply too much evolutionary distance between humans and our closest evolutionary relatives to make an investigation of homologous traits in any sense worthwhile. Furthermore, even though the genetic difference between humans and chimpanzees may be slight (they share about 98% of the same genetic material), mild quantitative changes may lead to large qualitative ones (Kennedy, 1992). Moreover, there are so few close relatives with which to compare humans (Lewontin, 1990).

These factors lead to the possibility that similar traits expressed by both chimps and humans may not reflect common evolutionary origins, but may instead be merely analogous in nature, and underwritten by very different causal processes (Rose et al., 1984; Sociobiology Study Group, 1978).

> Claimed external similarity between humans and our closest relatives (which are by no means very close to us) does not imply genetic continuity. A behaviour that may be genetically coded in a higher primate may be purely learned and widespread among human culture as a consequence of the enormous flexibility of our brain. (Sociobiology Study Group, 1978, p. 284)

If arguments based on homologous traits are problematic then those based on purely analogous reasoning may be doubly so. The Sociobiology Study Group (1978) suggests that arguments from analogy are on shaky ground unless the similarity between humans and animals is so precise that identical function cannot be denied. Sober (1993a) makes an important distinction in this context between *functional* and *accidental* similarity. The wings of birds and bats, for example, are functionally similar; they have evolved for same, or similar reasons, whereas the green coloration of ferns and lizards is an accidental similarity; they are the products of different evolutionary processes. Because many human traits *may* be maintained via cultural transmission rather than natural selection, there could be a high degree of superficial or accidental similarity between the traits of humans and other animals which is not due, in the case of humans, to evolutionary processes. To say, for example, that slavery in ants is analogous to slavery in humans is to ignore the very different causal processes which have given rise to the phenomenon in the two different species. Ant 'slavery' is maintained by hard-wired processes based on olfaction and probably represents a specific adaptation, whereas human slavery is a consequence of certain social forces which are unlikely to have any obvious evolutionary history. This kind of criticism, taken to its logical extremes, suggests that there is a decisive and unbridgeable chasm between the worlds of humans and that of other animals. As Reed (1978, p. 37) suggests, "Although humans retain features in common with the animals, once they created their own social and cultural institutions, they made a drastic departure from the animal condition and became non-animal, or human." A rather more modest conclusion is that we need to pay more attention to the causal processes which are responsible for the character in question before evaluating the worth of specific comparative claims (Kitcher, 1985, 1990; Sober, 1993)

The problem of identifying analogous and homologous traits between humans and other animals is further exacerbated, it is argued, by anthropomorphic thinking. It is claimed (e.g. Rose et al., 1984; Reed, 1978; Kitcher, 1985) that human concepts have been widely and fallaciously applied to other animals. Notions such as castes, slavery, polygyny, rape and so forth, it is asserted, are simply misapplied when used to describe the behaviour or social arrangements of other animals. Kitcher (1985, p. 184) suggests that "there may grow up a largely unexamined collection of unsubstantiated hypotheses latent in our linguistic usage, that allows us to pass freely from conclusions about non-human animals to conclusions about ourselves." This issue is not merely one of linguistic purity, argues Sober (1993b), but involves substantive claims about the way the world is.

Sober (1993a, 1993b) is especially concerned here that species of behaviour are so broadly defined that they are stripped of their full, real world complexity. Human concepts are pared of their rich relationship to social worlds in a manner which renders them meaningless.

> Sex and food are categories of human endeavor. Both categories include some behaviours that are unique to human beings and others that we share with other species. Sociobiologists typically use broad definitions of behaviour, so that the behaviour defined is not uniquely human. Much of what we include under the heading of sex and food in common parlance is thereby pushed into

> the shadows, as not part of the 'real' phenomenon at all. By broadening the definitions of the behaviours, we perhaps can obtain an *explanandum* that is tractable from an evolutionary point of view. The danger is that we may confuse this part with the whole from which it was extracted. There is much to food and sex that goes beyond the evolutionary explanation of why we eat and copulate. (Sober, 1993b, p. 203)

The transfer of human concepts to animals is doubly dangerous. Not only do we jeopardise the richness of the human characteristic as Sober suggests, but we also risk mistakenly attributing to animals concepts which are not entirely applicable. The widespread use of anthropomorphic thinking is labeled a 'disease' by Kennedy (1992), who argues that this way of thinking is built into us: we are predisposed to see human characteristics in other animals. Our evaluation of camels as haughty and robins as cute, for example, reflects (as Lorenz has argued) innate biases in our way of processing information about faces. Camels' nostrils are placed slightly higher than their eyes mimicking a disdainful expression while robins display the short face and large head characteristic of youth in humans. These are qualities that we perceive in other animals which do not necessarily accurately reflect their true natures.

To sum up these main ideas, criticisms of the use of comparative explanations in the evolutionary programme in psychology pivot on claims that (1) humans are simply too different from other animals to engender comparison (2) similar traits in humans and animals may reflect very different causal processes and (3) humans are prone to misapply human concepts to animals.

A limited defense of comparative explanations

The use of comparative analyses of animals' mind and behaviour plays an important role in developing evolutionary explanations in psychology. If it can be shown that such comparisons are invalid in *general,* then the explanatory power of the evolutionary research programme in psychology is considerably diminished. I will argue here, however, that the four uses to which comparative psychology is put can all be defended as valid in principle. The crucial point here is that we need to take a case by case approach to assessing the validity of comparative explanations (Kitcher, 1985;

Sober, 1993; Fisher, 1991). As long as we reject the idea that humans are so radically different from other animals in all respects, then we can retain the possibility that similar causal processes can be identified as being responsible for similar traits in humans and a wide variety of other species.

The question of continuity between humans and other animals is one with a long and acrimonious history. Darwin insisted that humans have no special place in the grand scheme of things, and that there are rich and detailed connections between the attributes of humans and those of other animals. These details reflected a history of common descent as well as the operation of similar evolutionary processes. Charles Lyell, on the other hand, as we have seen, while accepting the fact of evolution could not see how the obvious differences between humans and other animals could be bridged. This dispute regarding the uniqueness of humans remains prevalent in contemporary discussions on the value of comparative psychology. That humans *do* have characteristics which are not shared by other animals is indisputable. However, this is true of *any* animal species at some level of analysis. As the title of a book by Robert Foley (1987) suggests, humans are just *Another Unique Species*. It is difficult to see how this fact by itself requires alternative *kinds* of explanations to be developed for human characteristics.[1]

Anthropomorphism, as Fisher (1991) notes, is no obvious category error. It is not a *logical* fallacy to attribute human properties to other animals, or to assume that a single terms will encompass both the traits of humans and other animals. Indeed, the nature of evolutionary processes suggests that similarity in function due to similarity in causal process is a common feature of evolutionary history. Evolutionary processes, as Sterelny (1992) argues, are more robust than critics sometimes suggest. Many processes appear reliably across different species; convergent evolution is a relatively common phenomenon. Sterelny (1992, p. 100) suggests that this convergence should be just as apparent in behaviour as it is in form: "If similar evolutionary processes in similar environments can carve similar morphologies, the same must be true for behaviour".

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[1] Although it may be said, plausibly enough, that some unique human characteristics such as language, social learning and culture have created phenomena which are effectively removed from the biological sphere. I consider this possibility in a discussion of the nature of culture below.

What is important to isolate here is *proper function*. In terms of the realist approach to science that I advocate in chapter one, functional categories can be viewed as real in the sense that they carve the world at its proper joints. The biological category 'digestion', for example, carves the world at a functional joint. Despite the wide variety of organisms that can be said to engage in digestion, and various physiological ways of realizing this process, there is something real about the category 'digestion', and we can happily apply it to what goes on inside cows, humans, and venus fly-traps. What makes digestion a real, definable property of the world, is its biological function: the extraction of nutrients from the environments via external and internal processes of decomposition in order to obtain energy. There may well not be necessary and sufficient conditions for what is to count as digestion, but what is important in determining function is the selective forces in the environment acting upon the evolution of organismic systems. Function carves and is carved from the world.

So what is important in our attribution of similarity is the *causal* process which has given rise to the trait in question. Are such attributions easy to make when it comes to behaviour, or do other processes obscure proper function so that comparative analyses are mired on a pervasive causal indeterminacy? To reiterate, this can only be decided on a case by case basis. The distinction between homology, functional analogy and accidental analogy, however, *can* be made. There is no logical problem here, although the distinctions may be methodologically difficult to isolate. An example will help to clarify here, just how we can go about making the distinction between superficial and functional similarity.

In his book *The Evolution of Desire,* the evolutionary psychologist David Buss (1994) discusses the copulatory behaviour of the 'lovebug' *Plecia neartica.* Lovebugs remain in a copulatory embrace for three days. This is a male strategy to assure female fidelity and to increase paternity certainty. While attached to the female the male is preventing the possibility that other males will mate with the female. Other strategies are employed by different insects for the same effect, such as the use of vaginal plugs, mate guarding, and sperm scrapers. These different strategies attempt to solve the adaptive problem, common to all males, of paternity uncertainty. Buss (1994) goes on to say that this problem is solved in humans by the psychological mechanism of jealousy and the various behaviours that it motivates.

Buss is clearly not making a case for homology here. What he seems to be doing is employing the comparison with lovebugs as a heuristic device for bolstering claims about the evolutionary origins of jealousy in humans. The argument works like this:

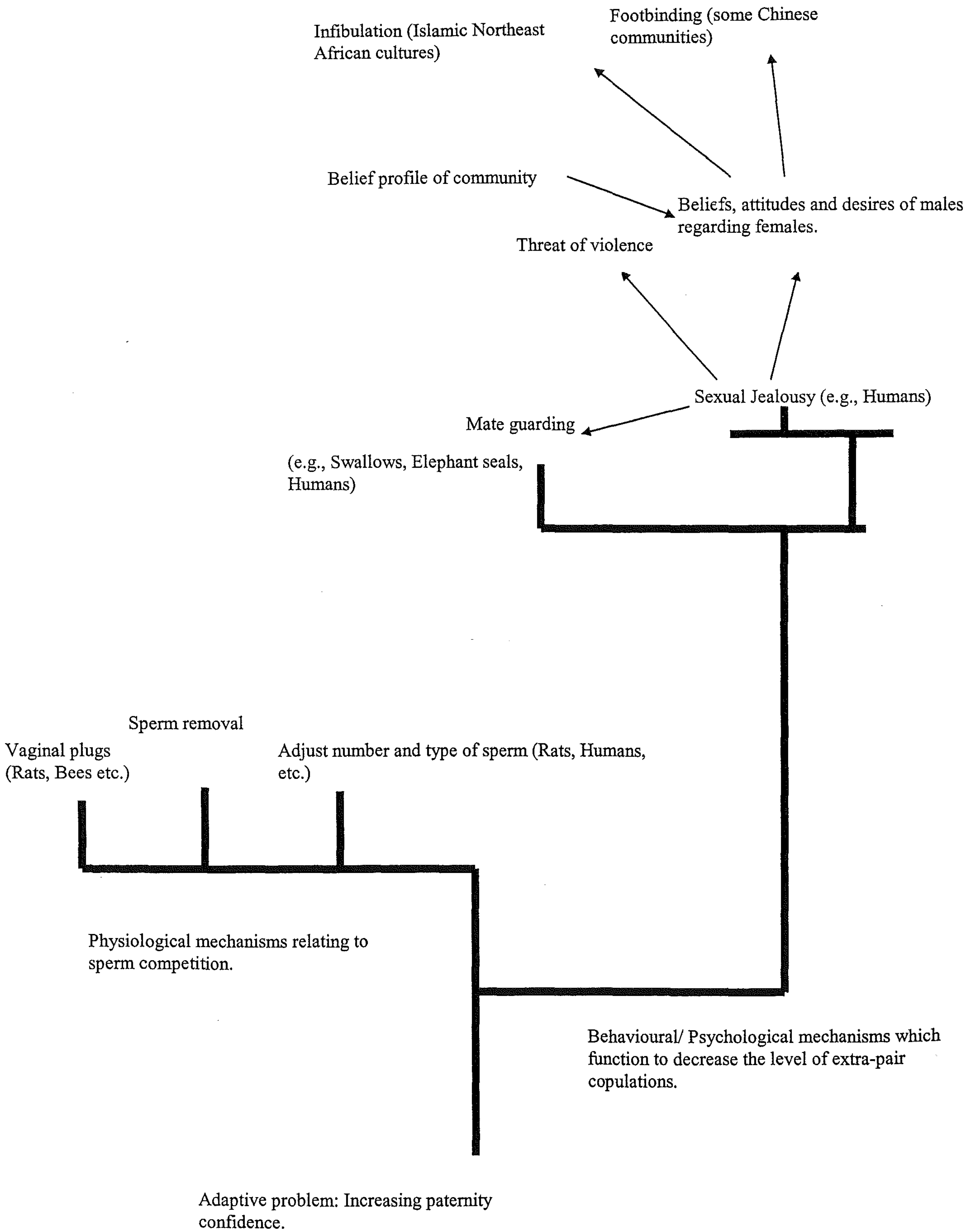
(1) There is a general problem, P, for males of all sexually reproducing species of retaining a mate and assuring paternity certainty.
(2) This problem is solved in species X via the mechanisms Y and Z.
(3) The mechanisms A and B in humans also seem to solve the general problem P.
(4) If the mechanisms Y and Z in species X have an evolutionary explanation, then this adds plausibility to the claim that mechanisms A and B in humans also have an evolutionary explanation.

The argument from analogy, therefore, is employed to elucidate the origins of quite different behaviour: the embrace of the lovebug and jealousy in humans. The argument hinges on functional rather than superficial similarity. As the focus of explanation becomes more similar, then stronger cases can be made for common evolutionary origins. For example, both male swallows (Moller, 1987) and human males (Flinn, 1988) engage in mate guarding. The males of both species spend more time with their mates when they are fertile than when they are not. Mate guarding, it is argued has evolved for similar reasons in both humans and swallows: it decreases the probability of extra-pair copulation and increases the paternity confidence of the male. Daly, Wilson and Weghorst (1982) discuss a variety of tactics that are employed by human males in different cultures such as the foot-binding of Chinese women, incarceration, and infibulation. They argue that all these practices should be understood as confidence-of-paternity mechanisms.

Can we really consider infibulation in humans to be analogous to the strategies of other animals, such as the use of vaginal plugs? It is easy to see that they both may be adaptive, but have they arisen due to the same *causal* processes? The answer to this question is probably no. Infibulation is almost certainly not an adaptation at all; it has not been selected *for* during the course of our evolutionary history. Alternative explanations would suggest that infibulation is the product, partly if not primarily, of certain cultural forces (e.g. Hicks, 1993). This issue touches on some core areas of concern regarding evolutionary explanations in psychology: how to identify adaptation, the role of competing explanations, and the appropriate level of analysis to employ. I shall confine myself here to a discussion of this issue as it pertains to the problem of comparative analysis.

Figure four outlines a variety of ways that males of different species attempt to solve the problem of paternity certainty. The diagram is incomplete, but I have sketched two main pathways in which increasing paternity certainty can be realised. One, through a variety of physical and physiological mechanisms and two, through a range of different behaviours. The possibility that humans and rats can adjust their levels and type of sperm depending on the likelihood of extra-pair copulation (Baker & Bellis, 1989, 1990) is *prima facie*, a strong case for functional similarity. Although the physiological mechanisms underlying sperm competition in rats and humans may well be quite different, they both can be furnished with a similar evolutionary explanation: those rats or humans who increased the number of sperm in their ejaculate in response to cues which indicate a recent copulation by the female with another male would have increased the representation of their genes in the next generation. Male sexual jealousy and the mechanisms underlying mate guarding are also plausibly analogous in the strong sense that they both reflect the action of the same causal process: evolution by natural selection. Mate guarding itself in both humans and swallows is more weakly analogous. Although superficially similar and no doubt both adaptive, mate guarding in humans is unlikely to be an adaptation in its own right, but instead one possible manifestation of psychological jealousy. The causal mechanisms underlying mate guarding in swallows and humans therefore are more diffusely related. Once we consider the case of infibulation, the analogy with other species all but breaks down. Although the practice may in some sense be related to male sexual jealousy, any explanation of the nature of infibulation, its geographical extent, its cultural significance and so forth, will need to draw heavily on the relevant cultural and historical forces. The causal histories of infibulation in humans and vaginal plugs used in many species of insects, reptiles and mammals, therefore, are

<u>Figure four</u> **Comparative causal taxonomy of phenotypes relating to the problem of paternity certainty**



likely to be distant enough, despite some superficial similarity, for the use of analogous reasoning to be unsound.

The use of analogous reasoning, therefore, can be deemed sound in some cases but not others. Similarly, it seems reasonable to apply the same term for animal behaviour as for humans in some contexts but not in others. In our development of concepts and the way that they map onto the world, we are always going to create categories of entities which are similar but variable. By a careful use of phylogenetic and ecological information, however, we should be able to reasonably define and limit the extent of the categories that we employ and the terms that refer to them. Although there may be much more to the concept of eating for humans than for cows, and much more to olfaction in dogs than humans, the categories 'eating' and 'olfaction' are capable of embracing the actions of multiple species.

Problems with cognitive explanations of animal behaviour

Now that the emphasis in evolutionary explanations of human behaviour has shifted towards an understanding of psychological mechanisms, the importance of developing an understanding of animal minds has come to the fore. Long banished as a legitimate source of scientific inquiry, the minds of other animals has now become an area of heated discussion, research, and some controversy. Since Donald Griffin (1978, 1984) resurrected the comparative psychology of Romanes (1881) and others twenty years ago, there has been a burgeoning of research in a variety of areas relating to animal cognition, intentionally, and consciousness (e.g., Walker, 1983; Dennett, 1983; Burghardt, 1985; Ristau, 1991; Cheney & Seyfarth, 1990; Previde et al., 1992). Can such a comparative approach to mind, however, be sustained? What can we possibly know (if anything) about the minds of other animals?

Critics of comparative explanations in evolutionary accounts of human behaviour argue that the problems of identifying analogy and homology and the general problem of taxonomy are especially acute in accounts of animal cognition (Lewontin, 1990). Moreover, it is argued (e.g., Kennedy, 1992) that humans are especially prone to over interpret animal behaviour as the consequence of complex mental processes when other explanations are more reasonable. Kennedy (1992) argues that comparative psychologists from Romanes to Griffin have followed an inappropriate methodology of extrapolating from human mind to animal mind via their behaviour. Similar behaviours in other animals, it is argued, can be performed without complex cognition. ". . . evidence that animals behave adaptably and adaptively is not evidence that they think consciously" (Kennedy, 1992, p.13). Kennedy (p. 31) advocates an epistemically cautious attitude to the attribution of mental states to other animals. "Although we cannot be certain that no animals are conscious, we can say that it is most unlikely that any of them are."

I certainly think there is a general problem here in determining the nature of psychological mechanisms in other animals, and over liberal interpretations of animal behaviour are easy to construct. However, as mentioned in the discussion of comparative explanations more generally, a case by case process needs to be adopted. Certainly we should not rule out, on any *a priori* basis, the possibility of mentalistic explanations of animal behaviour. Indeed, I believe we should adopt the same epistemic attitude towards our explanations of animal behaviour as we would any other phenomena. Given some pattern of behaviour in a given species, we should abductively generate a range of plausible explanations for that behaviour. At a distal level we may invoke evolutionary accounts of the survival and reproductive benefits of the behaviour concerned, while at a proximate level we should consider the kinds of mechanisms which might have evolved to realise these goals. In general there are three, not necessarily mutually exclusive, alternatives:

(1) *The physiological stance.* The behaviour is due to purely physiological processes which have been wired to respond to a certain class of environmental contingencies.
(2) *The behavioural stance.* The behaviour is a result of a history of learning and is explicable in terms of the processes of operant and respondent conditioning.
(3) *The intentional stance.* The behaviour is explainable in terms of the cognitive processes which are operating in the mind of the animal.

That we cannot *observe* mental processes should not be a barrier to their invocation in explaining certain patterns of behaviour. After all, many explanatory hypotheses in science draw on unobservable causal processes. We need to operate on a case by case basis and accept the *best* explanation for the pattern of behaviour to be explained.

There are certainly some good reasons to believe that animals *do* possess cognitive processes, and that an understanding of these processes is an important part of the evolutionary programme in psychology. Firstly, as Dupre (1996) notes, explanations of the behaviour of many animals under natural conditions are typically not best served by reference to purely behavioural or physiological processes. This is especially true once we consider the complex social behaviours exhibited by primates (e.g., deWaal, 1982, 1996; de Waal & Luttrell, 1987; Whiten & Byrne, 1988; Byrne, 1995). Secondly, if we accept the evolutionary continuity between humans and other animals, then we should expect that other animals, especially primates, possess some of the properties found in the human mind (e.g. de Waal, 1991). This point is reinforced by considerable similarity at both the neural and behavioural level between humans and other primates (Crisp, 1996).

Moreover, detailed studies of the minds of other primates, particularly chimpanzees, is likely to illuminate, *contra* Lewontin (1990), the evolutionary history of the human mind. Recent work by Povinelli and others (Povinelli, Nelson, & Boysen, 1990; Povinelli, 1993; Byrne, 1995) on the chimpanzee 'theory of mind', for example, suggests that chimpanzees share with humans some of the psychological mechanisms underlying the attribution of mental states to others. Although it is unclear that chimpanzees possess a full-blown theory of mind module (Povinelli & Preuss, 1995), a detailed account of what they *are* capable of provides a fruitful area for the elucidation of potential evolutionary pathways, and hence increases our understanding of this phenomenon in humans. Indeed the rudiments of an ability to attribute mental states to others may be seen in the putative ability of many species to be attentive to the gaze direction of other animals. For example, in a study on plovers, Ristau (1991) demonstrated that plovers were clearly sensitive to the gaze direction of a potential intruder and were more likely to perform distraction displays when this gaze was directed towards their nest. Attention to gaze direction emerges at an early stage in human development with infants sensitive to this dimension by six to twelve months (Butterworth, 1991).

Summary

Criticisms of the role of comparative explanations in the evolutionary programme in psychology do not, I would argue, significantly diminish its overall explanatory coherence. The use of analogical reasoning can be viewed as legitimate, although we must pay due attention to the relevant causal processes underling the traits to be compared. Furthermore, although humans posses some important attributes not found in other species, there is no reason to believe that humans, in general, are so radically different from other animals in all respects so as to warrant radically different kinds of explanations. Comparative psychology therefore, I believe, plays and will continue to play, an important role in the evolutionary programme in psychology. Furthermore, comparative approaches to mind are likely to prove as fruitful as those developed with reference to physiology or behaviour. In developing a coherent picture of the biological world, one which pays due attention to underlying and unobservable causal processes, and one which is consistent with evolutionary theory, we must draw on the full resources of the comparative approach.

Chapter seven

The use of adaptation explanations in the evolutionary programme in psychology

Evolutionary explanations of psychological phenomena draw heavily on the notion of adaptation. Sociobiologists and evolutionary psychologists discuss the function of the traits they study in terms of the putative survival and reproductive benefits they would have conferred over the evolutionary course of the species in question. Male sexual jealousy, for example, is conceptualised as a psychological mechanism with the proper function of decreasing the likelihood of extra-pair copulations by a male's mate, therefore increasing paternity confidence in subsequent offspring (Wilson & Daly, 1992). Similarly, the cheater-detection mechanisms studied by Cosmides & Tooby (1989, 1992) have the function of maintaining symmetrical benefits in the context of social exchange. The psychological mechanisms underlying jealousy and social exchange are viewed as adaptations; they have evolved because they further the survival and reproductive goals of those who possess them relative to others, in appropriate environmental contexts.

The use of adaptation explanations in psychology, however, has drawn a range of criticisms from a variety of sources. Many of the challenges to the use of adaptation explanations in psychology are based on more general criticisms of what is sometimes termed the adaptationist programme in evolutionary biology. In this chapter I consider the range of criticisms which have been directed at adaptation explanations in general before examining some of the specific criticisms which have been directed at the use of adaptation explanations in psychology. These criticisms can be conceptualised predominately as challenges to the overall explanatory breadth of the evolutionary programme.

Adaptation and natural selection

At the heart of the theory of natural selection, the core of the evolutionary research programme, is the notion of adaptation. For evolution to occur, different traits of organisms must have different fitness values. These traits which confer fitness benefits to organisms are termed adaptations. Or, more precisely, adaptations are traits which have been selected for because of their role in furthering the survival and reproductive success of organisms that possess them, and hence in increasing the frequency of those traits in the population relative to other traits.

Understanding the causal processes that have led to the evolution of particular adaptations provides an answer to the pervasive 'why questions' directed at biological entities.[1] Why do mammals have eyes? Why are flowers brightly coloured? Why do birds sing? Are all questions which demand an answer in terms of the selective pressures which have given rise to the trait in given environments. Mammals have eyes in order to see, brightly coloured flowers attract pollinators, and birds sing to attract mates and to define territories.[2] These questions are all teleological in nature; they request an answer in terms of the *function* of the item in question.

There is a crucial distinction to make here between something's function and *accidental* properties of the same item. The heart has the function of pumping blood, for that is the property of the organ by virtue of which it continues to be maintained in the population of organisms with hearts. The fact that the heart also beats, is red, and so on, are accidental or non-functional properties of this organ. This etiological theory of function (Wright, 1973; Millikan, 1984, 1986, 1989, 1993) takes the distinction between accidental and causal properties of an item to be understood by reference to historical processes: the selective forces which have shaped and maintained some aspects of biological characters over others during the course of evolutionary history. Sober (1984, p. 100) makes an important distinction here between selection *for*, and selection *of*: " ' Selection of' pertains to the *effects* of a selection process, whereas

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[1] There are also of course 'how questions', in terms of the proximate physiological and psychological mechanisms underlying the trait in question.
[2] These answers here are all very vague, but as will become apparent, the answers to our 'why questions' can, and should be, as rigorously formulated as possible.

'selection for' describes its *causes.*" So the heart has been selected *for* a blood pump, while there has been selection *of* beating, redness, and so on.

This etiological notion of function can be distinguished from accounts of function which focus on the dispositional properties of items. Bigelow and Pargetter (1987) articulate a theory somewhat along these lines. An item has the function it does when it confers a survival-enhancing *propensity* on an organism that possesses it. Their theory is forward looking in that even before an item is selected (by virtue of its function) that item can be said to have that function. On the other hand, a character which is no longer survival-enhancing, say the coat of a polar bear in a globally warmed future, will not be considered to have the function of keeping the animal warm anymore.

The dispositional theory of function, however, fails to make the crucial distinction between current utility and evolutionary function. It is therefore unable to provide normative guidelines as to what is to count as abnormal or malfunctional. Moreover, it fails to adequately address the why questions that we want to ask of biological entities by ignoring the historical processes which have given rise to the traits in question.

History is important because function can only be forged in the crucible of selection. Using the terminology developed by Millikan (1984, 1987, 1989, 1993), an item has the *proper* function it does by virtue of its natural selection over other items, due to the functions it has *Normally* performed during the course of evolution, which have contributed to the survival of the organism, and hence the differential reproduction of those genes (in part) responsible for that item. "Normal" is understood by Millikan not in the sense of average or even statistically likely, but more in relation to ideal or optimal conditions: ones that have at some time in the organism's past contributed significantly to the selection of the character due to its functioning in those conditions. An item may not, indeed need not, fulfil its function all the time for it to attributed with that function. Many biological traits will be of this nature. The proper function of the croak of a male frog, for example, is to attract female frogs in order for reproduction to occur. However, the average or indeed normal (not Normal) male call probably does not achieve this effect. There may be no females in the vicinity for example, or there may be females about but they mate with caller's neighbor, or the caller (or his potential mate) is eaten before the call achieves its proper function.

The idea of Normal conditions highlights the important role of the environment in elucidating adaptations. A trait is only adaptive in the context of the appropriate environment. Therefore, a full understanding of the function of any item can only be achieved by an analysis of the ecological conditions in which the item in question is embedded, during the process whereby the trait has been, and continues to be, selected for. There is a mesh here between the nature of the trait and the environment of which it is a part. Function is, therefore, necessarily an environment relative concept.

Challenges to adaptationism

The etiological account of function detailed above and its relation to the concept of adaptation and the theory of natural selection, could be considered the received view in evolutionary biology (Brandon, 1990). However, in recent times there has been much debate over the power of natural selection and hence the relative importance of adaptive reasoning in explaining the morphological, behavioural, and psychological characteristics of organisms. The hegemony of adaptive explanations in evolutionary biology has been challenged in a series of papers by Stephen Jay Gould, Richard Lewontin, and colleagues (Gould & Lewontin, 1979; Lewontin, 1978; Gould & Vrba, 1982; Gould, 1980a, 1980b, 1991a, 1991b, Eldredge, 1995).

The critique of what Gould and Lewontin (1979) term the adaptationist programme or 'Panglossian paradigm' pivots on what is perceived as an inappropriate and over extensive use of adaptationist reasoning in explaining the origin of biological characters. Evolutionary biologists in general and sociobiologists in particular, it is argued, are prone to constructing 'just so' stories of organismic traits which trade on a naïve plausibility based on the misguided notion of the omnipotence of natural selection (Gould & Lewontin, 1979; Lewontin, 1979; Gould, 1989; Caplan, 1989). Furthermore, these just so stories, it is claimed, are unfalsifiable and produced *ad hoc* to explain biological phenomena. For if one 'just so' story fails, it is the strategy of the adaptationist to simply fashion another one, until ultimately, in this procrustean fashion the explanation fits. Generally speaking, it is argued, there simply will not be adaptive explanations for many organismic traits; not all structures have a *why*, many only have a *how*.

The critique presented by Gould and Lewontin (1979) comes in four interrelated parts. Firstly, adaptationists inappropriately atomize the traits of organisms into discrete parts. Secondly, adaptationists assume a kind of Panglossian optimality with respects to the traits of organisms. Thirdly, adaptationists ignore other evolutionary forces in their explanations of biological phenomena. And lastly, adaptationists confuse current utility for historical origin in their attributions of function. Before I examine these criticisms in more detail, it is important at this point to distinguish the content from the rhetoric of this challenge to adaptationism (see Dennett, 1995, for a further elaboration of this point). At times it appears as though Gould and Lewontin want to forsake adaptation explanations entirely, in favour of other evolutionary forces. This is not the case. Their critique is a challenge to the *power* of natural selection and to the methodology employed by adaptationists. As such, they do not propose an alternative to natural selection as the core of the evolutionary research programme, but instead, provide some plausible ways of augmenting that core with auxiliary theories and alternative methodologies.

Adaptationists, so Gould and Lewontin (1979) argue, have the tendency to atomize organisms into discrete traits. However, no biological item exists in isolation; it is necessarily part of a larger system. Breaking an organism up into discrete parts and telling a selective story about each of these traits is ignoring this fact. The human chin for example, is not a thing (let alone a thing with a function), but just the by-product of an interaction between two growth fields. Just as the spandrels of St. Marks cathedral are necessary architectural by-products of mounting a dome on rounded arches, so to will there be many spandrels to be found among biological items, which are by-products of more specific biological design (Gould & Lewontin, 1979). This problem of demarcating adaptations from by products of adaptations is, as we shall see, especially acute in evolutionary explanations in psychology, and is part of a more general problem of identifying the natural 'suture lines' of organisms (Lewontin, 1979), especially organisms with any degree of phenotypic plasticity. Out of the myriad of phenotypic expressions of any organism, which traits can we point to and say that they have a proper function and a selective history, and which traits are mere effects of adaptive design?

I shall examine the important problem of identifying adaptations in more detail below, but it is worth noting now that even by-products have, as it were, adaptive explanations. That is, the nature of the by-product itself can be explicated by reference to the appropriate adaptations. Consider the extraordinarily maladaptive behaviour of 'flame-seeking' among moths. Our (just about) mythical arch sociobiologist may attempt some kind of 'just so' story to explain this behaviour, perhaps in terms of helping to incubate eggs, sexual titillation, or the luring of rival moths to a fiery evolutionary dead-end. But this kind of explanation is clearly neither necessary nor helpful. In a world before naked flames were common, small bright sources of light would have represented escape holes from hollow logs or caves, or celestial bodies at optical infinity (Dawkins, 1982). This suggests several plausible adaptive explanations for the light-seeking behaviour of moths: escape, navigation, or perhaps both. Flame-seeking behaviour therefore has no function, but itself can be explained by reference to behaviour that does. The problem of carving the natural world at its functional joints is then, I would argue, predominately a problem of *specificity*: that is, identifying the *level* at which the character in question has been selected.

The second major line of criticism directed at the adaptationist programme by Gould and Lewontin (1979) is their charge of Panglossian reasoning in explicating the character of biological traits. According to the Panglossian paradigm this must be the best of all possible worlds; organisms are manifestations of this optimism and are literally *optimal.* Or as Gould (1991d, p. 60) expresses it: Panglossianism is "the notion that everything must fit, must have a purpose, and in the strongest version, must be for the best". A major theme of much of Gould's writing (e.g., Gould, 1989b, 1991) is the role of historical contingencies on the current character of living organisms. "Pathways of history. . . impose such jury-rigged solutions upon all creatures. History inheres in the imperfections of living organisms. . ." (Gould, 1991d, p. 61). Mother nature, as Jacob (1977) aptly expresses it is a tinkerer not a designer. All organisms are the cobbled together products of many millions of years of contingent evolution. As a consequence, the design of many organisms are sub-optimal, certainly in terms of what a mythical creator might have been able to conjure up given enough time, materials, and foresight. These 'senseless signs of history' (Gould, 1980) are everywhere. The upside-down vertebrate retina, the crossover of the digestive and respiratory system in the mammalian throat, and the circumlocutory route of the human male urinogenital tract are all sub-optimal legacies of the evolutionary process (Williams, 1992). Gould's (1980a) favourite example is the evolution of the panda's 'thumb'. As members of the order *carnivora*, the panda's fingers had evolved from ancestors who used them for running and scratching, not grasping and manipulating. The switch from a carnivore's to a vegetarian's diet and the subsequent need to manipulate food items resulted in the evolution of a grasping digit actually constructed from the radial sesamoid bone, part of the panda's wrist.

Other kinds of constraints on optimality include developmental constraints, available genetic variation, and time lags in tracing environmental change (Dawkins, 1982). Developmental constraints as Maynard-Smith, Burian, Kauffman, Alberch, Campbell, Goodwin, Lande, Raup, and Wolpert (1985, p. 266) define them are biases " on the production of variant phenotypes caused by the structure, character, composition, or dynamics of the developmental system." Allometry, for example, is an especially relevant kind of developmental constraint. Allometry refers to a yoked correlation in the character of two variables (Williams, 1992). Parts of organism which may seem to serve no purpose may actually be the consequence of modifications in correlated parts. Panda's for example, as well as having a thumb constructed from an enlarged radial sesamoid bone also have an enlargement to the tibia sesamoid bone in the foot. This enlargement serves no proper function and is just the consequence of enlargement to the wrist bone with which it is developmentally yoked (Gould, 1980a).

A lack of optimal fit between an organism and the environment may also be the result of time lags. Evolution is often too slow to track rapid environmental change (Dawkins, 1982). The kakapo for example, a three kilogram flightless New Zealand parrot with low breeding rates, may not be adaptive in a post-human environment which includes stoats, rats, and dogs, but in a predator-free environment flightlessness, gigantism, and low-fecundity will not be sub-optimal. The importance of teasing apart current environments from those which organisms have evolved in is particularly crucial in isolating adaptive mechanisms in humans. This is especially relevant since humans have, in some respects, radically altered their environments over the last ten thousand years or so. In general the problem of time lags emphasises the role that Normal conditions play in identifying the proper function of biological traits.

The presence of constraints on perfection should serve as an important reminder of the role of historical and developmental factors on the evolution of organismic traits. The presence of such constraints, however, does not by itself, invalidate the central features of the adaptationist programme (although it may serve to eliminate more extreme versions). Sewall-Wright's metaphor of the adaptive landscape is useful for understanding the relevance of this point. If we conceive of design space as a three dimensional topographical map with hills and mountains representing adaptive peaks, then we see that selection can not redesign; it can not cross the less adaptive valleys[3], it can only push parameter values towards local optima. These peaks in the mountain range may not represent the *best* solution to adaptive problems, but the are the best *available* to the organism given its particular evolutionary trajectory.

> Like a river, natural selection blindly meliorizes its way down successive lines of immediately available least resistance. The animal that results is not the most perfect design conceivable, nor is it merely good enough to scrape by. It is the product of a historical sequence of changes, each one of which represented, at best, the better of the alternatives that happened to be around at the time. (Dawkins, 1982, p. 46).

The third line of criticism against adaptationism provided by Gould and Lewontin (1979) concerns the role that other forces apart from natural selection have to play in the explanation of biological phenomena. Not all biological items will have proper functions, because forces other than natural selection are causally responsible for their existence. Other agents of evolutionary change include pleitropy, genetic drift, chance, and phyletic and ontogenetic constraints.

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[3] Although this will generally be the case, if selection pressures are sufficiently relaxed, mountain peaks can be descended, valleys crossed, and higher peaks scaled (Dawkins, 1996).

Genetic drift is a stochastic process which occurs in small isolated populations where the frequency of alleles may be fixed by random factors rather than by selective forces. The role of genetic drift in the production of biological characters is a matter of some controversy. However, both Dawkins (1982) and Williams (1992) argue that it is unlikely that drift plays a large role in the fixation of maladaptive traits. Pleitropic effects occur when a gene is causally responsible for the production of more that one trait. One of these traits may be adaptive while the other is maladaptive. I think it would certainly be a mistake to call the maladaptive trait an adaptation, although as in the moth example outlined earlier, it may ultimately be explained by reference to an adaptation: the functional trait whereby it gains its foothold in the organism.

One important developmental constraint, mentioned earlier, is allometry, which may also be causally responsible for character traits which have no manifest function. According to Gould (1991a), the extremely large egg of the kiwi relative to body size[4]is a classic example of a trait whose origin lies in allometric scaling rather than simple adaptation to ecological circumstances. In asking the question why do kiwis have such large eggs, Gould suggest that we should go beyond answers which rest with what the egg is good for and address the historical origin of large eggs in kiwis. It is likely that kiwis evolved from much larger ancestors. As body size reduces so too does egg size, but not to the same extent. The large size of the kiwi's egg, therefore, is simply a consequence of dwarfing coupled with normal allometric scaling of egg to body size. This example provides useful lessons in the importance of establishing the historical origin of current traits. However, it fails to invalidate the role of adaptive explanations in general and more specifically in this example. Whatever the evolutionary *origin* of a given trait is, it should still count as an adaptation as long as it is *maintained* by natural selection. Kiwis may have large eggs relative to their body size because their ancestors were considerably larger and their subsequent dwarfing resulted in a relatively lower decrease in egg size as expressed through allometric scaling principles. However, the maintenance of this trait in the population is likely to due to the fact that larger eggs produce more precocial chicks which can survive for longer periods without food early in life. As well as being the largest egg relative to body size, the kiwi's egg is also the most nutritious, with a higher percentage of yolk

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[4] Kiwis' eggs range up to 25 % of the female body weight at 400-435 grams. For a bird the size of a kiwi one would expect an egg size of 55-100 grams.

than any other egg. The absence of predators in the New Zealand environment allowed this development to take place by not handicapping the egg bound female relative to those birds who tended to lay smaller eggs. Whether or not the consequence of allometric scaling is likely to be adaptive should be taken on a case by case basis. The enlarged tibia sesamoid bone of the panda, for example, is likely to a truly non-functional part of that organism's phenotype.

This case by case approach should also be adopted when assessing the causal role of developmental constraints more generally. This is nicely illustrated in another of Gould's (1991b) essays 'male nipples and clitoral ripples'. Gould argues that an explanation of male nipples and female orgasms is most appropriately sought from an understanding of growth and development rather than an application of adaptation by natural selection. Male nipples certainly seem to have no function and even the most imaginative of pop-sociobiologists in unlikely to produce even a vaguely plausible 'just so' story for their possible adaptive role.

> Male mammals have nipples because females need them - and the embryonic pathways to their development builds precursors in all mammalian foetuses, enlarging the breasts later in females but leaving them small (and without evident function) in males. (Gould, 1991b p. 127).

Gould (1991b, p. 129) adopts a similar position on the putative function of the female orgasm: " The reason for a clitoral site of orgasm is simple - and exactly comparable with the non-puzzle of male nipples." However, I think in this case Gould's imagination has failed him. There *are* some good reasons to believe that female orgasms do have an evolutionary function. For a start, the sensitivity of the clitoris is three times greater than that of the homologous penis: a difference which is unlikely given a non-functional hypothesis. Moreover, the cervical contractions produced during orgasm provide a means to rapidly upsuck male sperm deposits and speed their way to the female ova. Baker and Bellis (1995) in their book *Human Sperm Competition* have argued that the timing and frequency of the female orgasm is a means of manipulating the likelihood of conception with any given partner. Orgasm functions therefore as a kind of internal means of sperm screening. Whether or not this explanation is true remains an open question, but one which is surely worth pursuing in more detail.

Structural constraints on organisms' phenotypes caused by phylogenetic inertia and non-functional legacies of past evolution are also likely to produce character traits which are non-adaptive. Vestiges of course are the classic example, and most animals will carry around atrophied and non-functional products of past evolutionary processes. The general role of history in explaining character traits lies behind Gould's insistence in distinguishing between current utility and historical origin. Indeed, Gould and Vrba (1982) have proposed a change in terminology to characterise this distinction.

Gould and Vrba (1982, p. 6) make what they see as a crucial distinction between adaptations and what they term *exaptations*.

> An adaptation is any feature that promotes fitness and was built by selection for its current role. Characters evolved for other usages (or for no function at all) and later co-opted for their current role are exaptations.

Only adaptations have functions, exaptations merely have effects. The evolution of wings and feathers in birds is used as an example in distinguishing adaptation from exaptation. Feathers originally evolved as thermoregulatory devices, but were later (with suitable modification) co-opted for flight, whereas wings' original function was for insect catching, and more latterly for powered flight (see Ostrom, 1974). Thermoregulation then is the function of feathers, while catching insects or flight are the mere effects of this prior function; they are exaptations. As feathers were co-opted for the new purpose of flight, modifications arose in the structure of feathers, the wings, and (presumably) the neuro-cognitive mechanism underlying flight behaviour. These modifications Gould and Vrba (1982) term secondary adaptations. Any complex character, it is suggested, is going to be a (in principle distinguishable) mixture of adaptations, secondary adaptations, and exaptations.

The distinction that Gould and Vrba (1982) make between exaptation and adaptation is problematic. Is it really meaningful to talk about feathers as exaptations for flight, if they have been independently maintained by selection for this function? In the case of feathers, there has been selection for a modification of structure, so that feathers function within the context of flight. Given that all biological items have some past proto-items as their base, how far should we take the item back to uncover its real function; what should count as the primary adaptation? I agree with Gould here that it is important to demarcate the current use of an item form its historical origin and proper function, but I would argue that this falls naturally from the etiological account of function outlined earlier. As Griffiths (1992a) points out, the notion of vestige is important here. If a trait no longer serves a function, then typically it will begin to atrophy. The rapid loss of sight and pigmentation among cave dwelling animals is a good example of this. Vestiges may, however, be maintained by natural selection for their role in organising embryological development (Griffiths, 1992a).

It is certainly important to demarcate the current use of an item from its historical origin and proper function. We need to be able to distinguish the evolved function of a trait from selectively irrelevant by-products, or spandrels. However, I would argue that this distinction falls naturally from the etiological account of function outlined earlier without recourse to additional terminology. Some trait will have a function, that is, be an adaptation, if it was originally selected for that function or it is *maintained* by selection for that function regardless of its evolutionary origin (Griffiths, 1992a; Brandon, 1990). Traits which are currently useful or used for novel purposes, but have neither been selected for nor maintained by natural selection, will properly be considered as by-products, or effects of selection processes.

A general defense of adaptationism

Clearly evolutionary biology could not proceed without some reference to function or adaptation without a radical change to the hard core of the evolutionary programme itself. As is suggested earlier, Gould, Lewontin, and colleagues, despite the hype surrounding their critique of adaptationism, do not dispute the truth of natural selection; they only question its relative *importance* in explaining biological phenomena.

A pragmatic defense of adaptationism is provided by Mayr (1983), Dennett (1983), and Dawkins (1982). These authors argue that although not all traits will be adaptations this should be the first line of explanation for any complex biological character.

> " [The evolutionist] must first attempt to explain biological phenomena and processes as the product of natural selection. Only after all attempts to do so have failed, is he justified in designating the unexplained residue tentatively as a product of chance. (Mayr, 1983, p. 326.)

As Dawkins (1982) notes, adaptationism as a working hypothesis has led to some outstanding discoveries. Von Frisch's path-braking work on colour vision in fish and bees, for example, proceeded on the basis that colours in fish and bees must be *for* something (other than the delectation of human senses). Ultimately, as Sober (1984, 1993) emphasises, it is an empirical question (although not always one *settled* by the 'facts'),whether or not a given character is the product of natural selection or is only a by-product of that process, or has arisen by chance.

Assuming that many but not all traits will be adaptations, and that current function is often a poor indicator of proper function, how then can we go about deciding where function exists? Out of the myriad of potential phenotype expressions of any organism (especially those which are relatively plastic) how are we to decide which are real, functional, adaptive properties of the organism, and those which are so much noise; spandrels, not amenable to functional explanations?[5]

> Clear signs of a selectionist origin include complexity of design, with 'heterogeneous structure' producing a 'unity of function' that can be 'stated independently and more economically than description of the structure' and that is 'special because it specifically benefits the organism that has it' (Millikan, 1993, p. 4.)

The eye, for example, is clearly an adaptation for sight. The eye's complexity precludes the possibility that all the elements which go in to producing integrated

[5] Although, as I argue above, some spandrels although not adaptations themselves can be furnished with satisfactory causal explanations by reference to adaptations.

function occurred by chance factors alone. Although the physical details of eyes vary considerably between different species (It is estimated that the eye has evolved independently over forty times [Dawkins, 1996]), the function of eyes remains invariant. Eyes benefit the organisms that possess them by providing them with rich and detailed information about the world.

A related approach to identifying adaptations is the comparative method. Convergent evolution is a familiar phenomenon to evolutionary biologists and strongly suggests, in the absence of uniform phyletic constraints, that the trait converged upon has some adaptive explanation (Harvey & Pagel, 1991). For example, one unusual characteristic of the New Zealand flora is the high number of divaricating plants that it contains. Divaricating plants are " small-leaved shrubs or tree juveniles which characteristically have a wide branching angle, as a result of which the branches form an interlaced, three-dimensional structure with a relatively leafless exterior" (McGlone & Webb, 1981, p. 20). New Zealand has fifty-four species of divaricating plants which belong to twenty genera, representing seventeen families - some ten per cent of the entire woody flora. In addition a number of trees go through a juvenile divaricating stage before reaching maturity (Greenwood & Atkinson, 1977). Because of the high proportion of divaricating plants, their representation across different species, and their rarity outside of New Zealand, it is assumed that there must be some kind of adaptive explanation. "Any hypothesis as to the origin and evolution of divaricating plants must therefore explain the adaptive significance of their structure and show why this life form is rare in other parts of the world." (McGlone & Webb, 1981 p. 20)

Deciding that something has a function (i.e., selective history), however, is but the first step in uncovering what that function might be. Two main hypotheses have been put forward to account for the proliferation of divaricating plants in the New Zealand flora. McGlone and Webb (1981) argue that divarication is a climatic adaptation, which arose during tree-less periods of glaciation in the Pleistocene, to protect growing points and leaves from wind abrasion, desiccation, and frost damage. The alternative explanation, championed by Greenwood and Atkinson (1977), is that divarication arose as an adaptation in response to browsing by moas (a guild of now extinct New Zealand birds). It is not my place to make a decision in this debate (the evidence for the two hypotheses seems fairly evenly balanced), but to note that often it is clear that something is an adaptation, without necessarily knowing what that something is an adaptation for.

Many putative adaptation explanations are of the kind outlined above: they strongly suggest some proper function for the item in question without unequivocally delineating what that function is. Typically speaking, too many pieces of the explanation are missing to make any definite decision on what the function of the item in question is. Brandon (1990, p. 163) argues that there are five components to an ideally complete adaptive explanation:

(1) Evidence that selection has occurred.
(2) An ecological explanation of the fact that some types are better adapted that others.
(3) Evidence that the trait in question is heritable.
(4) Information about the structure of the population from a genetic and selectionist perspective.
(5) Phylogenetic information concerning primitive and derived characteristics.

Almost no current adaptive explanation will fill all of these criteria to everyone's satisfaction. However, explanations that fall short of the ideal, are not therefore epistemically worthless. Incomplete explanations provide us with an increase in understanding of the trait in question and serve to stimulate inquiry so as to fill in the explanatory omissions. A further distinction is made by Brandon (1990) between *how possibly* and *how actually* explanations of biological phenomena. Most adaptation explanations trade on their plausibility; they are how possibly explanations. But such explanations have a cognitive value that is independent of whether or not they reflect the way the trait actually evolved.

This point I think goes some of the way in rebutting the charge of adaptationist story telling. Adaptive claims are certainly not falsifiable in rigid Popperian terms, but many theories in science, and particularly theories in the life-sciences, are going to be of this nature. It is certainly not true that modifications to adaptive claims are just so much *ad hoc* story telling. After all, the story is not arbitrarily chosen, and it *is* independently testable. The strategy of trying another hypothesis after the first has failed (as Mayr, 1983 notes) is a usual methodological practice in all branches of science.

A more general point here is that we should be looking for the *best* explanation (Harman, 1964; Thagard, 1992) for any given phenomenon. Our initial strategy when confronted with a biological trait to be explained is to abductively generate a range of plausible[6] explanations, of which adaptive explanations will figure prominently but not exclusively. As we generate more information regarding the phenomenon in question, we can evaluate our alternative explanations in terms of their overall explanatory coherence (Thagard, 1992). As I have emphasised in the first chapter of this thesis, our most explanatory coherent explanations may well not be true (indeed they will surely be false in *some* respects), but it will be epistemically rational to retain this explanation until a better one is proffered. An active search for alternatives (in the manner of Gould, Lewontin, and others) is surely an important part of this process, but we should not reject plausible accounts of biological phenomena unless we have alternatives which are more explanatorily coherent.

I have examined the concept of function and the role of adaptive explanations in some detail here because these issues are particularly relevant to an evaluation of evolutionary explanations in psychology. At the heart of many critiques of the evolutionary programme in psychology are challenges to the use of adaptationist reasoning in explaining psychological phenomena. In particular, it is argued that many of the manifestly observable characteristics of humans are non-functional and therefore defy an evolutionary analysis.

The use of adaptation explanations in the evolutionary programme in psychology

Adaptation explanations are used widely in evolutionary biology to explain the characteristics of many animal species. The use of such explanations in the context of human psychology, however, has drawn a variety of criticisms. The criticisms of

[6] Plausibility here can be thought of in terms of generalisations that we believe to be true, such as natural selection itself and the relevant auxiliary theories of the evolutionary programme.

adaptation explanations in the evolutionary programme in psychology comes in three broad categories:

(1) *Methodological.* It is argued that there is widespread and inappropriate invocation of adaptation on the basis of scant evidence. Moreover, adaptation explanations in human psychology are constructed *ad hoc*, and are unfalsifiable in nature.
(2) *Taxonomic.* The taxonomic criticism suggests that the traits which are invoked as adaptations are at the wrong level of analysis. That is, there is an arbitrary partitioning of the phenotype which does not reflect evolutionary design.
(3) *Alternative explanations.* Many, if not most, of the interesting phenomena that psychologists study, it is argued, will not have adaptation explanations as they are the consequence of other causal factors such as learning and culture, or are merely the by-products of a cognitive system which has evolved for other reasons.

In general these criticisms can be seen as challenges to the methodological adequacy and explanatory breadth of the evolutionary programme in psychology. If it can be demonstrated that it is methodologically implausible to use adaptation reasoning in psychology and/or that much of what psychology studies (and should study) bears no relation to biological adaptations, then the scope of the evolutionary programme will be radically attenuated: evolutionary explanations will get no explanatory purchase on psychological phenomena, and other types of explanations will be favoured. In this section I will discuss these three general kinds of criticism in turn, and clarify what I take to be the role that adaptation explanations have to play in psychology. It will be argued that adaptation explanations in psychology can be (but not always are) methodologically acceptable, and that while falling short of explanatory omnipotence, adaptation explanations provide us with an increased understanding of a wider range of psychological phenomena than some critics allow.

Methodological problems

Adaptation explanations in psychology and the social sciences have been charged with a pervasive panglossianism (e.g., Gould, 1989a; Sociobiology Study Group, 1978; Lewontin, 1979; Rose et al., 1984; Kitcher, 1985; Flanagan, 1991). It is suggested that sociobiologists and evolutionary psychologists attempt to explain every aspect of the human phenotype as being evolutionarily advantageous. For example, Flanagan (1991, p. 278) criticizes Wilson for trying to find an adaptive advantage in all human characteristics. "In both *Sociobiology: The New Synthesis* and *On Human Nature* Wilson tells one Panglossian story after another. For instance, he cites the religiously sanctioned cannibalism of the Aztecs as a cultural response to the genetically programmed need for protein". Similarly Rose et al. (1984) argue that there is a fallacious conflation of what is possible with what is optimal, and that "Sociobiology is Pangloss made scientific through the agency of Charles Darwin".

There are two general points of contention here. One, that not all human characteristics are the product of natural selection. And two, that evolutionary orientated scientists engage in facile storytelling rather than constructing detailed arguments. The first point stresses that we should not expect that behaviour and morphology to be the inevitable result of natural selection and that there are other potential causal factors in operation (Gould, 1989a, Gould & Lewontin, 1979, Lewontin, 1979). The second point claims that sociobiologists are spinners of unfalsifiable "just so" stories, with "Virtuosity in invention replacing testability as the criterion for acceptance" (Gould, 1989a, p. 530). The approach adopted in developing evolutionary explanations of human characteristics is described as "imaginative reconstruction" (Lewontin, 1979), with the simple invention of reasons why some traits may have selective advantages. As the Sociobiology Study Group (1978, p. 28) bemoan: "The trouble with the whole system is that nothing is explained because everything is explained. If individuals are selfish, that is explained by simple individual selection. If on the contrary they are altruistic, it is kin selection or reciprocal altruism". Rose et al. (1984) make a similar point in claiming that the combination of direct selection, kin and reciprocal altruism provides unlimited explanatory resources which are "insulated from any possibility of being contradicted by fact" (p. 261). Indeed it is argued that sociobiology is so methodologically defunct that "it does not have a claim to be a serious form of scientific investigation" (Lewontin, 1979, p. 6).

At the heart of these criticisms are disputes about the standards of evidence which are deemed acceptable by evolutionary scientists. Kitcher (1985) urges for greater rigour in the identification and analysis of adaptations in human psychology. "Conclusions about adaptation" Kitcher (1985, p. 177) quips " are not to be established by free association". Lewontin (1979, 1990) makes a similar point in highlighting the extreme epistemic problems in reconstructing the origins of past adaptations. What is required to demonstrate proper function, Lewontin argues, is not that some trait *might* have been favoured by natural selection but that it *was* favoured. To this end we need details about the genetic basis of the trait, the genetic variation for the trait in the population, and evidence that the trait in question has contributed to the past reproductive success of those who possess it. These problems, Lewontin (1990) argues, are especially acute in evolutionary explanations of human psychological characteristics. Firstly, because the genetic evidence required is unlikely to be forthcoming, and secondly, because the reconstruction of the psychological profiles of our ancestors is likely to prove impossible. Lewontin (1990, p. 229) concludes: "We know essentially nothing about the evolution of our cognitive capabilities and there is a strong possibility that we will never know anything about it"

In summary, the methodological criticism of the evolutionary programme in psychology claims that adaptation explanations in psychology are subject to a fallacious Panglossianism, are unsupported and insupportable by the evidence, and are unfalsifiable in nature. I think it is important to disentangle two different interpretations of these criticisms which are sometimes run together. The first interpretation suggests that evolutionary explanations of human psychological phenomena are *necessarily* invalid because they are impossible to either adequately verify or falsify. The second interpretation is a claim about the actual practices of sociobiologists and evolutionary psychologists who are charged with typically constructing evolutionary stories on the basis of limited or non-existent evidence.

I certainly think there is something to the second claim here, and that adaptation explanations need to be more rigorous at times. Indeed there has been a tendency in early evolutionary accounts of human behaviour to commit many of the errors outlined above. However, the claim that the evolutionary programme in psychology is bankrupt due to *inherent* methodological problems is unwarranted and cannot be sustained. As I have elaborated earlier, there are many reasons to believe that not all aspects of all phenotypes are likely to be adaptations, let alone the consequence of optimal design. Changes in the environment, developmental and phylogenetic constraints, trade-offs, conflicts of interest, and so forth will all lead to organisms which are not the gleaming product of designed foresight. Moreover, the operation of other evolutionary forces and the subsidiary effects of adaptations will mean that not all morphology can be given clear-cut adaptive explanations. A large part of the problem here is taxonomic in nature. As discussed in chapter three, and elaborated upon below, the *level* at which the adaptation is identified is important. Given the flexibility of human behaviour and the plurality of environments that humans inhabit, items of behaviour are unlikely to be good candidates for adaptations, although they may be explained by them. Ritualised cannibalism by Aztecs is unlikely to be an adaptation, therefore, however adaptive the practice might be (to some parts of the society at least), although the desire for protein embodied in meat may well be. In the evolutionary study of phenotypic characteristics there needs to be a realisation that not all characteristics are likely to be adaptations. Although guilty therefore of an inappropriate Panglossianism at times, the evolutionary programme in psychology is by no means committed to such an approach, and more recent accounts have avoided some of the more egregious errors of this nature perpetrated by earlier investigations.

The claim that evolutionary explanations explain too much and are unfalsifiable in nature has also been discussed earlier. The persistence of this critique, however, warrants a more detailed response. The first important point to make is that general research programmes are not subject to direct empirical disconfirmation. What is under test, therefore, is not the general evolutionary research programme or even sociobiology or evolutionary psychology, but the specific hypotheses which have been generated from those research programmes. As elaborated in chapter one, the hard core of a research programme is immune from falsification by virtue of the protective belt of auxiliary theories. The theories of reciprocal altruism and kin selection, therefore, rather than providing an illicit means of explaining 'everything' as Rose et al. (1984) claim, are merely an extension of the hard core of the evolutionary programme, and do real explanatory work. It would seem to me that the explanatory riches of the evolutionary programme should be considered an epistemic virtue, not a source of embarrassment.

While downplaying the role of testability as the *sine qua non* of a scientific theory, it is still important that theories have empirical consequences and that certain states of the world can be seen as incompatible with specific theoretical claims. Typically speaking, the specific claims of sociobiologists and evolutionary psychologists do have important empirical consequences and certain states of the world can be seen as incompatible with specific theoretical claims. For example, the theory of natural selection, coupled with principle of natural selection suggests that there should be a difference in the cues that elicit feelings of sexual jealousy in human males and females. Because of paternity uncertainty, males should be more sensitive to indicators of sexual jealousy on the part of their mate, while females should be more concerned with cues suggesting that their partners are diverting resources to other females (Buss, 1994). This leads to the specific and highly testable hypotheses that males will report more jealousy and greater autonomic nervous system arousal, imagining a situation of sexual infidelity, while females will experience greater jealousy and more arousal imagining their partners spending more time with another female. Of course, if the results did not pan out as expected, it would not be rational to abandon the principle of natural selection or even the theory of parental investment. However, any modifications that were made of the basis of the pattern of results would need to be both independently testable and coherent with the rest of the programme.

The epistemic problem of unequivocally demonstrating adaptation is certainly not an easily tractable one. Recalling the distinction between 'how possibly' and 'how actually' explanations (Brandon, 1990) made earlier in this chapter, we can align adaptation explanations along a spectrum of epistemic plausibility regarding the specific causal forces which are responsible for the traits which are being explained. While conceding that many explanations of human characteristics have fallen squarely at the 'how possibly' end of this epistemic continuum, there is no need for global pessimism regarding the possibility of developing increasingly plausible adaptation explanations of human characteristics.

As discussed earlier, adaptations can be identified by a number of different means. Typically adaptations will be species typical, well organised, mesh with the environment, and be characterised by special design features such as economy, efficiency, complexity, precision, specialisation and reliability (Tooby & Cosmides, 1990b). Complex mechanisms, as Dawkins (1986) emphasises, do not arise by chance, or as by-products of other processes, but instead are evidence of design or of a process - natural selection - which mimics design. The comparative method can also be deployed in identifying adaptations that are shared with different species because of either common descent or convergent evolution.

An idealised complete adaptation explanation (Brandon, 1990), will include, among other things, details about the genetic basis for the trait, genetic variation in the population, and the reproductive success of those bearers of the traits under Normal conditions. As Lewontin (1979, 1990) notes, there is nothing like this kind of detail for any extant evolutionary explanation in psychology. However, this is not a problem peculiar to psychology and we have very few complete adaptation explanations for *any* biological trait. There are several reasons for this. Because natural selection uses up genetic variation, there is unlikely to be substantial genetic differences between individuals in a species, especially where there is a considerable degree of interbreeding, as in humans. The heritability of most adaptations, therefore, is likely to be zero (Tooby & Cosmides, 1990a). At the current state of genetic science we also have no detailed information about the genetic basis of most biological traits, especially those which involve the operation of many genes as is the case with most behaviours.[7]

Accumulating the necessary evidence that a given trait has been selected for because it has been advantageous to the organisms that possess it, is also difficult to achieve. One approach, often adopted by behavioural ecologists (e.g., Clutton-Brock, 1988), is to measure the actual reproductive success of contemporary populations of organisms which vary on some given characteristic. This is also the approach to identifying adaptations advocated by some human behavioural ecologists (e.g., Borgerhoff-Mulder, 1987; Caro & Borgerhoff-Mulder, 1987). Caro and Borgerhoff-Mulder (1987) argue that measuring reproductive success or correlates of reproductive success is a more epistemically viable option then constructing explanatory accounts of past, and observationally opaque, selection pressures. There is no doubt that the methodological problems of identifying adaptations are lessened by studies of

[7] Although this may not always be the case. Moreover, it is possible to get some idea of the potential genetic basis of a given trait by comparing individuals who have genetic abnormalities which impair their performance on given tasks.

contemporary populations. However, as I have outlined in chapter three, evolutionary psychologists (e.g., Symons, 1989, 1990; Cosmides & Tooby, 1987, Tooby & Cosmides, 1990b) have cogently argued the measures of current reproductive success can only tell us about present directional selection and cannot inform us about past selective pressures. As the data accumulated by Vining (1986) illustrates, putative correlates of reproductive success in past environments such as status and wealth do not lead, in Western populations at least, to greater reproductive success. Current reproductive success, in some environments, may be useful as a heuristic device, but the focus in identifying adaptations should be on those underlying design features which imply a selectionist origin (Tooby & Cosmides, 1990b).

There are clearly many methodological barriers in developing complete adaptation explanations in psychology. These problems, however, do not warrant a global pessimism regarding the evolutionary programme in psychology, as Lewontin and others suggest. Lewontin (1979, 1990) argues that all adaptation explanations can only be considered plausible storytelling. This is true. Because adaptation is necessarily a historical concept, and because in the absence of time travel technology we cannot directly observe the past, our hypotheses regarding adaptation cannot be vindicated by the appropriate data regarding genetic differences and reproductive success. This does not mean, however, that we should abandon the whole explanatory enterprise embodied in the adaptationist programme.

Lewontin's conclusions are based, I would argue, on an insupportably stark and inadequate empiricist philosophy of science. Such a philosophy emphasises the role of hypothetico-deductive method and counsels a scientific approach based on the generation of hypotheses and their subsequent evaluation in a manifestly observable world. Such an approach, as I have argued in the first two chapters, fails to do justice to either the actual practice of good science or the underlying complexity of the world itself. It is important to note that many sciences rely on explanatory hypotheses regarding the action of unobservables in both spatial and temporal terms. Sub-atomic physicists study elementary particles via indirect measures, cosmologists develop theories of unobservable historical events such as the big bang, and paleontologists speculate about causes responsible for mass extinctions. In all these cases the explanatory theories are appreciably underdetermined by the evidence and are not directly *confirmable* via observation. In the case of theories about the big bang or mass extinction no crucial experiment can be employed to distinguish between rival theories (although the theories may well have different empirical consequences). Evolutionary reconstructions of psychological adaptations are in a similar epistemic position. Our theories of past cognitive function are grossly underdetermined by the available evidence, and there is no way of directly *testing* the alternative theories because observational evidence is not readily accessible. However, this should not necessarily be cause for despair here, anymore than it is in cosmology or paleontology. A methodological approach to science ground in abductive method suggests that a plurality of theories should be generated in order to *explain* the range of relevant phenomena in the domain. Our best epistemic strategy is to evaluate the range of relevant theories in terms of *multiple* criteria, such as those embodied in Thagard's theory of explanatory coherence. It should be then possible to choose the *best* explanation from a range of alternatives.

Given that complexity of design is a good indicator of natural selection, it is reasonable to attempt to develop adaptation explanations of highly integrated, complex psychological phenomena. Other explanations of those phenomena, not based on the process of natural selection, will also be generated. At any given moment in the explanatory history of the phenomenon, it will be more rational to pursue one explanation over the other. The degree of epistemic acceptability of the explanation will be measured in terms of various general criteria for theory appraisal, of which empirical adequacy is only one.

The evolutionary programme in psychology, therefore, does not flounder on intractable methodological difficulties. A case by case approach would indicate that at any given time adaptation explanations will be more or less plausible than alternative accounts of the psychological phenomena in question.

The problem of taxonomy

Human behaviour is highly flexible. The vast array of potential phenotypic expressions in humans dwarfs that of any other species. As many have noted (e.g., Kitcher, 1985, 1988, 1990; Rose et al. 1984; Lewontin, 1979) this poses a potential problem for the evolutionary programme in psychology. The problem is one of identifying the natural 'suture lines' of evolutionary dynamics (Lewontin, 1979). That is, out of the vast array of phenotype expressions manifest in humans, which are true adaptations, and which are merely the by-products of adaptations or the consequence of other causal processes?

Sociobiologists have been criticised for employing arbitrary agglomerations of behavioural traits (e.g., Rose et al. 1984). Rose et al. (1984) argue that putative adaptation explanations of slavery, dance, cannibalism, and so forth fail to adequately identify natural kinds. The problem here is that such an approach ignores underlying mechanisms (Kitcher, 1985, 1988, 1990; Cosmides & Tooby, 1987; Tooby and Cosmides, 1989). What has been selected *for* by evolutionary processes (as elaborated in chapter three) is not instances of behaviour *per se*, but entire suites of developmental processes, including psychological mechanisms, which interface with the environment in such a way as to produce, under Normal conditions, adaptive behaviour. In developing adaptation explanations of behaviour, therefore, it is important to advert to the underlying mechanisms which produce the behaviour in given environmental contexts.

Many adaptations in most organisms are, to some extent facultative, not fixed in nature. Animals often have multiple strategies for interfacing with the world depending on context. It follows that in identifying adaptations in other animals, and especially in humans, the appropriate level of analysis must be sought at the level of the underlying mechanisms, for it is at this level which selection has occurred. This point is recognised in most contemporary accounts in the evolutionary programme in psychology (Symons, 1989, 1990; Cosmides & Tooby, 1987; Tooby & Cosmides, 1989, 1992; Buss, 1995).

Alternative explanations: the scope of adaptation explanations in psychology

There is general agreement that the human mind is the product of natural selection. Indeed, in the absence of alternative causal processes this must be the case. However, there is considerable dispute over the explanatory import of this fact. Critics of the evolutionary programme in psychology argue that the capacities that human minds possess overwhelm the explanatory significance of their origin by natural selection. There are three general, related, points to this line of argument. One, culture provides an alternative explanation for adaptive behaviour; two, evolutionary theory can only explain the operation of very general cognitive processes and therefore gains no explanatory purchase on specific psychological phenomena; and three, there is much *more* to human phenomena than can be explained via evolutionary theory. Taken as a whole, this line of criticism attempts to reduce the explanatory coherence of the evolutionary programme by reducing its explanatory breadth, and by positing potentially more cogent alternative explanations for psychological phenomena.

Culture has often been invoked as an alternative explanation to evolutionary theory for apparently adaptive human phenomena. As Gould (1989a, p. 532) explains: ". . . the existence of adaptive behaviour in humans says nothing about the probability of a genetic basis for it or about the operation of natural selection." Because humans have an alternative transmission system - culture - apparently adaptive traits may have arisen by trial and error and spread through the population by learning. The universality of a given behaviour or psychological characteristic, moreover, is no proof of its selectionist origin. As Dennett (1995) argues, cultural universality *may* be the product of reinvention by organisms with similar psychological characteristics. The simultaneous appearance of agriculture in MesoAmerica and the Nile valley about ten thousand years ago, for example, requires no explanation in terms of genetic changes or selective pressures.

Apparently adaptive behaviour, therefore, may not reflect the presence of underlying adaptations, but may merely be the product of cultural forces. Related to this point, the problem of developing adaptation explanations of human psychological phenomena is exacerbated, argues Gould and others (e.g., Gould, 1989, 1991; Kitcher, 1988; Flanagan, 1991), because evolution is likely to have built very *general* capacities such as the ability to form beliefs and desires. *Specific* adaptation explanations for a range of human psychological phenomena, therefore, may not be plausibly constructed. Gould (1991c, p. 55) utilises his concept of exaptation to forward this point.

> I wish to present an argument for regarding the human brain as, *prima facie*, the best available case for predominant exaptation - in other words, for a near certainty that exaptations must greatly exceed adaptations in number and importance.

Explanations for human psychological characteristics, therefore, suggests Gould, are not typically speaking going to be adaptation explanations. Wallace (1869), as discussed in chapter three, viewed the vast range of human abilities as greatly exceeding our basic survival and reproductive needs. As a hyper-selectionist, committed to seeing function in all traits, Wallace was pushed into the position of denying an evolutionary origin for the human mind (Gould, 1980c). Gould suggests that the concept of exaptation and the notion of spandrels, allows us to understand the extended repertoire of human abilities in a manner commensurate with, but not explicable by, natural selection.

> Natural selection built the brain; yet, by virtue of structural complexities so engendered, the same brain can perform a plethora of tasks that may later become central to culture, but that are spandrels rather than targets of the original natural selection. (Gould, 1991c p. 57)

For example:

> The universals of language are so different from anything else in nature, and so quirky in their structure, that origin as a side consequence of the brains extended capacity, rather than as a simple advance in continuity from ancestral grunts and gestures, seems indicated. (Gould, 1989c p. 14)

In conclusion, Gould (1991c p. 58) is emboldened to suggest " The concept of exaptation provides a one-line refutation of sociobiology"

The problem highlighted by Gould is one of specificity and comes in the strong version outlined by Gould above, which challenges the notion that even apparently central human cognitive processes like language might be considered adaptations, and weaker versions which question the explanatory role of evolutionary theory in accounting for the myriad of specific beliefs and practices which humans possess and engage in.

Moreover, because such beliefs play an important role in the cultural significance of much human behaviour, adaptation explanations will fail to provide an increase in our understanding of the full panoply of psychological phenomena. As Sober (1993) has suggested, there is more to food and sex, than an evolutionary analyses of eating and copulating might indicate. Psychologists, according to Sober (1985), will end up studying many traits which have no proper function. Sober (1985, p.190) argues that " We must relativise our ascriptions of functions to certain privileged capacities of the containing system." Only certain aspects of human psychology, therefore, might be amenable to evolutionary explanations.

The problem of specificity in adaptation explanations provides a challenge to the explanatory breadth of the evolutionary programme in psychology. If, as Gould suggests, only the very *general* aspects of the human mind have a selectionist origin, then the scope of evolutionary explanations in psychology will be considerably diminished. Even if a larger range of psychological phenomena are admitted as adaptations, the weaker version of the specificity argument suggests that we will get limited explanatory purchase on the kinds of things that we want a science of psychology to explain. Evolutionary explanations, if the specificity argument is sound, will only play a strictly limited role in our understanding of psychological phenomena.

In what has become a slightly repetitive theme, I would suggest that questions regarding the validity of adaptation explanations in psychology must be taken on a case by case basis. For each putative adaptation explanation we should evaluate its explanatory worth in comparison with alternative theoretical accounts. Having said that, I believe that there are good reasons to reject the strong version of the specificity argument espoused by Gould. However, weaker versions of the argument may well play an important role in delimiting the scope of evolutionary explanations.

Firstly, it is important to clarify Gould's claims with respect to some of the terminology that he employs. This is important, because it is my impression that social scientists often employ the term 'exaptation' in the context of discussing spandrels or by-products of adaptations rather than in the situations originally delimited by Gould and Vrba (1982). Some of this confusion is abetted by Gould himself who lumps together exaptations, spandrels, and by-products of adaptations in the same epistemic bag. However, as I have made clear earlier in this chapter, exaptations *are* adaptations. There *is* an important distinction to be made between the proper function of an item and subsequent uses of that item; however, this distinction is not illuminated by the notion of exaptation. It is hard to see, therefore, how exaptation provides a one line refutation of sociobiology even if exaptations in humans do vastly out-weigh adaptations in numbers. Indeed my point above suggests that such a comparison is otiose.

The argument, which does potentially challenge the explanatory power of the evolutionary programme in psychology, is the claim that natural selection has favoured a general purpose mechanism - the mind - which, due to its plasticity, can fulfil a vast array of different functions. These functions, however useful in nature, will not be adaptations, but will be the by-products or spandrels of an organ designed for other reasons. Gould is sure that the number of spandrels or by-products will vastly outweigh, in number and significance, the proper functions of the mind. Just why Gould is so sure is unclear; there is no detailed argument to support his claim and no *a priori* reason why adaptations in the realm of the human mind should be few in numbers and result in so many unintended and undesigned effects.

The strong form of Gould's argument flounders on his favourite example of a spandrel: human language. Gould (1989d, 1991) and others (e.g., Chomsky, 1972, Piattelli-Palmarini, 1989) argue that language is not an adaptation but instead is a by-product of other evolutionary forces which have led to an increase in brain size and can therefore be explained in terms of general laws of structure and growth.

Others however, have argued that human language is *prima facie* an excellent candidate for an explanation in terms of evolved function (e.g., Pinker & Bloom, 1990; Pinker, 1994; Dennett, 1995). The complexity of interrelated parts which operate together to achieve the single function of communication of propositional structure over a serial channel, strongly suggests that language was specifically designed by natural selection for this function (Pinker & Bloom, 1990; Pinker, 1994).

It is highly improbable that the mechanisms underlying articulate vocal control, auditory processing of speech, the construction of grammar, and the physiology of the mouth and larynx, all serendipitously emerged together as the product of other, more general processes.

As figure five suggests, at our current state of knowledge, the best explanation for language is the theory that it is an evolutionary adaptation with the specific function of communication of propositional structure over a serial channel. The claim put forward by Gould and others, that language is merely the spin-off of an overwhelmingly domain-general mind is difficult to sustain. As Pinker and Bloom (1990, p. 720) explain:

> " There is no psychologically realistic multi-purpose learning program that can acquire language as a special case, because the kind of generalisations that must be made to acquire a grammar are at cross-purposes with those that are useful in acquiring other systems of knowledge for example."

Moreover, the dissociability of language from other cultural and cognitive systems in both developmental and neuro-physiological terms is strongly indicative of domain-specificity, not of some general cognitive learning ability.

It is certainly true that we have nothing like a complete adaptation explanation for language at the current state of knowledge. However, the hypothesis that language is an adaptation is far removed from the weakest of 'how-possibly' explanatory accounts. Because natural selection uses up genetic variation we should not expect qualitative differences in the genetic basis for language acquisition in humans. However, evidence for specific language deficits (Gopnik, Dalalakis, Fukuda, Fukuda, & Kehayia, 1996), and qualitative variation in people's grammatical ability (Pinker, 1994), are suggestive (although far from conclusive) of an underlying genetic basis for language.

Detailed and plausible scenarios of language evolution have also been constructed (e.g., Corballis, 1991; Davidson & Noble, 1989; Dunbar, 1993; Aiello, 1996).

Figure five Explaining language: Alternative approaches

Explanations for language

(1) Language is a by-product of other evolutionary forces. It is not a specific adaptation but is the result of general laws of structure and growth and has spread via cultural transmission. (Gould, 1991, Chomsky, 1972).

Explained by:

Unknown laws of growth. General mechanisms not elucidated.

(2) Language is a domain specific biological adaptation with the function of communicating propositional structures over a serial channel. (Pinker & Bloom, 1989; Pinker, 1994)

Explained by:

Selective advantages of language for hominids under pleistocene ecological and social conditions.
(e.g. Corballis, 1991; Dunbar, 1993; Aiello, 1996).

Language phenomena to be explained
(from Pinker & Bloom, 1989; Pinker, 1994)

- Language is universal. All humans are proficient language users.
- All languages use the same basic mechanisms and representations.
- No correlation between language complexity and technological progress.
- Selective impairing and sparing of language functions relative to other cognitive processes.
- Brain areas dedicated (primarily) to language processing.
- Specialised physiology of the larynx which compromises other functions.
- Children fluent speakers of language by age three without formal tuition.
- Language learning maturational process with critical period for acquisition.
- Specialised processing in auditory perception which decodes speech into linguistic segments.
- Variability in language fluency among different speakers.
- Local differences in type of language spoken.
- Evidence for specific, genetically based language disorders.

Although these accounts differ in their explanations of just when, how, and why language arose, there is general agreement that language is an adaptation with the specific function of communication, and that its phylogenesis can be reasonably elaborated. The current, selectively relevant function of language may well be the result of co-option of structures used for other purposes[8]; however, a strongly plausible case can be made for the reproductive benefits of a communication system with increasingly greater expressive power. So, although our adaptation explanation for language remains a 'how possibly' one, it is also the most explanatorily coherent explanation for language phenomena and as such is the one it would be the most rational to accept.

The strong specificity argument proposed by Gould, therefore, seems difficult to sustain. From one example, of course, one cannot reject the possibility that the mind is *predominately* a general purpose machine whose effects far outstrip it functions. However, as discussed earlier, there are both good conceptual and empirical reasons to believe that the mind contains, along with general mechanisms, an array of domain-specific processes. A number of these processes, e.g., natural history intelligence (Atran, 1990), language (Pinker, 1994), mental state attribution (Baron-Cohen, 1995) moreover, can be formulated with plausible adaptation explanations. This suggests that evolutionary explanations will have *some* role to play in psychology. The extent of that role, however, remains an open question and relies to a large degree on the validity of the weaker versions of the specificity argument.

Weaker versions of the specificity argument propose that a large number, but not all, psychological phenomena which psychologists attempt to explain are not evolutionary adaptations, nor are they readily explicable in terms of underlying adaptations. The problem, it is suggested, is one of *grain* (e.g., Kitcher, 1988; Sober, 1993). Much of what is of interest to psychologists and social scientists, it is claimed, is so far removed from evolutionary dynamics that explanations need to be furnished predominately, if not exclusively, in terms of other causal processes.

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[8] Calvin (1993), for example, develops the plausible idea that language mechanism have been co-opted from ones underlying sequential processing, especially as related to throwing.

It is important to distinguish here between the claim that most psychological phenomena are not adaptations and the claim that most phenomena cannot be adequately *explained* by adaptations to some degree or other. Evolutionary psychologists are typically happy to concede the first point; however they do claim that many phenomena, although not good candidates for adaptations, may well be satisfactorily explained in terms of proper function, at least in part. For any organism with some degree of behavioural plasticity, diverse phenotype expressions may well be understood in terms of underlying adaptation across varying environmental contexts.

An important point to note here is that adaptations are usually *relational* in nature (Millikan, 1993; Sperber, 1996). "To have biological functions an item need neither have the same categorical *properties*, e.g., the same absolute structure, as items that participated in the life cycles of ancestors, nor need its *functions,* when categorically described, be functions performed by any of its ancestors" (Millikan, 1993, p. 173). Millikan asks us to consider the imprinting mechanism of ducklings. Although the mechanism has the proper function of forming a relation to the 'mother of' the duckling, the specific content of the imprint will be different for each duckling (all mothers are unique). The important point to note here is that the specific content of most internal states can only be understood with reference to those particular features of the environment which gives rise to those states. Those features will naturally be variable (individuals have different histories), but the variability is only explicable with reference to the mechanisms which underlie such variability (Tooby & Cosmides, 1989). A complete explanation for most phenotypic characters, this suggests, must typically advert to both underlying adaptation mechanisms and features of the environment during individual development.

A simple example will help to clarify this point. Humans in many contemporary cultures have a preference for cakes, chocolates, and Coca-Cola. This preference can be given an adaptation explanation even though the manifest behaviour is both novel and maladaptive. The explanation adverts to those feature of human olfaction and gustation which have evolved to enjoy the taste of food substances high in sugars. In ancestral environments the presence of sugar would have been indicative of a highly nutritious food source such as fruit. The abstract structure of the argument runs as follows:

(1) Mechanism P produces trait Q in environment Z. (Mechanism P is selected for due to the reproductive benefits conferred by trait Q in environment Z).
(2) Mechanism P produces trait N in environment W. (The same mechanism produces different [perhaps maladaptive] behaviour under different environmental circumstances).
(3) To explain trait N we refer to the mechanism P, producing the trait, and why it produces this trait in environment W. This explanation, although drawing on local features of the novel environment, will be, *prima facie*, an adaptation explanation.

Of course, most examples in psychology will not be so simple. However, the sheer presence of novel behaviour, variable phenotypes, and heterogeneous environments do not by themselves invalidate the possibility of constructing explanations in which reference to natural selection plays an important role. A useful distinction in this context is the one employed by Sperber (1990, p. 5) between actual and proper domains: "The *actual domain* of a conceptual module is all the information in the organism's environment that may (once processed by perceptual modules and possibly by other conceptual modules) satisfy the module's input conditions. Its *proper domain* is all the information that it is the module's biological function to process."

The proper domain of the sweet-detecting mechanisms in humans is likely to have been predominately fruit. In contemporary environments the actual domain includes all those sweet food items which satisfy the input conditions of the module and will include, among other things, cakes, chocolate and coca cola. In this example, knowing something about the proper domain gives us certain explanatory resources to account for phenomena in the actual domain of interest. For many areas of interest in psychology, however, this may not be the case. As Sperber (1994, p. 54) explains:

> . . . the actual domain of *any* human cognitive module is unlikely to be coextensive with its proper domain. The actual domain of any human cognitive module is sure, on the contrary, to include a large amount of cultural

> information that meets its input conditions. This results neither from accident, nor from design. It results from a process of social distribution of information.

We can, perhaps, understand patterns of social transmission via a detailed elaboration of psychological mechanisms and their proper function[9]. However, in many cases the actual phenomena of interest may be so insulated from those causal processes embodied in adaptation by natural selection, that an evolutionary explanation for the phenomena becomes untenable. In other words, alternative explanations, in terms of culture, or the belief-desire profiles of individuals *screen off* adaptation explanations from the phenomenon in question. The notion of screening off suggests that one class of explanation makes another class statistically irrelevant. In the context of the present discussion proximate causes are suggested to screen off ultimate ones. This issue is discussed in more detail in the final chapter.

It is possible to construct a continuum of phenomena in which adaptation explanations play an increasingly less or more important role. For example, consider music. No clear and well articulated adaptation explanation has been constructed for the origin of music. It is plausible to suggest, however, that an explanation for music construction and appreciation is the actual domain of the proper domain for language (Sperber, 1996). Because the acoustic properties of music satisfy (perhaps super-satisfy) the input conditions of language, people have the necessary discriminative ability and motivational force to create and enjoy music. Our derived adaptation explanation gains us some explanatory purchase on the phenomenon of music: It may explain the universality of music, why some acoustic properties of music are preferred over others, why we enjoy bird-song, why some scales make us feel happy and others sad and so on. Once, however, we want to explain the emergence of jazz music in the 1920's in America or the transition from bebop to cool in the 1950's, our adaptation explanation does little if no explanatory work. In the case of music, Sperber (1996) argues the cultural domain is often much more salient and relevant than the proper domain. Many critics of evolutionary explanations in psychology would argue that the example of music highlights a *general* problem for the evolutionary programme in psychology. For example Dennett (1995, p. 491) suggests:

[9] This point is elaborated in more detail below, but as Sperber (1994, p. 54) points out the factor which effects the distribution of ideas is their "compatibility and fit with human cognitive organisation".

> Whereas animals are rigidly controlled by their biology, human behaviour is *largely* determined by culture, a *largely* autonomous system of symbols and values, growing from a biological base, but growing indefinitely way from it. *Able to overpower or escape* biological constraints in most regards, cultures can vary from one another enough so that important portions of the variance are thereby explained . . . . Learning is *not* a general-purpose process, but human beings have so many special-purpose gadgets, and learn to harness them with such versatility, that learning *often* can be treated as if it were an entirely medium-neutral and content-neutral gift of non-stupidity.

The first point to make regarding this general problem is that in psychology, as in other sciences, we should be trying to develop complete, vertically integrated (Barkow, 1989) explanations of psychological phenomena. These explanations will draw on accounts of the myriad of causal processes which are likely to be responsible for the phenomena in question. The question for psychology, therefore, is to what extent certain causal processes are likely to do the most explanatory work. This, of course, can only be adjudicated on a case by case basis. In reference to the explanatory breadth of the evolutionary program in psychology there are three alternative tiers of phenomena in which adaptation explanations play more or less of a role:

(1) Psychological phenomena with fairly clear selectionist origins and coherent adaptation explanations which furnish us with a deeper understanding of the phenomenon in question.
(2) Phenomena in which selectionist accounts provide us with some explanatory purchase and therefore remain relevant, but which also require considerable causal input form predominately non-selectionist mechanisms such as cultural evolution.
(3) Psychological phenomena that are so far removed from the evolutionary process so as to virtually eliminate the explanatory worth of selectionist explanations.

The explanatory scope of the evolutionary programme in psychology will be determined to a large extent by the number of phenomena which fall in to class one and two compared to class three. While trying to avoid any sweeping claims here, I would suggest that for *psychology* at least, most, but certainly not all of the phenomena of interest, fall into classes one and two. For sociology, and to some extent cultural anthropology, the phenomena of interest may well accumulate predominately in tier three and hence be less amenable to adaptation explanations.

Summary

In this chapter I have argued that adaptation explanations have a legitimate role to play in evolutionary biology in general and in evolutionary explanations in psychology more specifically. Whether or not something should be considered an adaptation should be taken on a case by case basis. That is, we should compare adaptation explanations with alternative accounts in terms of their explanatory coherence in explaining the phenomena in question. The role of evolutionary explanations in psychology will be determined, in part, to the extent that the phenomena that psychologists study are adaptations or amenable to adaptation explanations. My discussion suggests that there are some clear examples of psychological adaptations in humans which can be furnished with epistemically plausible adaptation explanations. Moreover, many of the traits which characterise contemporary human populations may be explained, in part, by reference to underlying adaptations.

Chapter Eight

Explaining diversity: Culture and learning as alternative explanations

Nativism vs. Empiricism: The role of learning and the environment in evolutionary explanations.

As many have noted, the proposed dualism between nature and nurture has a long and acrimonious history. In psychology this dispute has centered on whether the rich repertoire of human and other animal behaviour is the consequence of learning mediated by the social and physical environment, or is the result of innate, genetically determined developmental processes. In discussions of the validity of the evolutionary programme in psychology the schism between nature and nurture has been framed in terms of alternative causal explanations. On the one hand, evolutionary social scientists have been characterised as advocating a form of 'bean-bag genetics' where behavioural traits are tightly linked to the genes that purportedly 'code' for them (Sociobiology Study Group, 1978; Rose et al., 1984). While on the other hand, mainstream psychologists are seen as promulgating a view in which the forces of learning and culture mould individual behaviour in a way not explicable by biological processes.

This dispute regarding the role of different causal forces in explaining human behaviour remains an important one, even though there is wide-spread agreement that genetic and environmental factors are reciprocally related during development and that it is inappropriate to *oppose* biological and learning explanations (e.g., Rose *et al.*, 1984; Cosmides & Tooby, 1987; Tooby and Cosmides, 1992; Sterelny, 1992; Sober, 1993). As Tooby and Cosmides (1990a, p. 19) point out: "Anyone with a biological education acknowledges that the phenotype is the result of the interaction between genes and environment, and all aspects of the phenotype are equally co-determined by this interaction."

If there is widespread agreement on the thoroughly epigenetic nature of development, then why do questions of determinism remain to the fore in discussions of the validity of the evolutionary programme in psychology? One reason, as discussed earlier, probably lies with the ethical concerns which are associated with evolutionary explanations of human behaviour. In the lay public's mind at least, the invocation of evolutionary arguments are still, to a wide degree, associated with inevitability, and perhaps acceptability. For psychologists, however, I think the predominant issue at stake is one of explanatory *primacy*. Although there may be agreement on the reciprocal role of genes and the environment during development, there is considerable dispute over which causal factor exerts the greatest influence and hence provides us with the best explanation for the phenomenon in question. In many ways the dispute here is misguided. If genetic and environmental factors are mutually interdependent, then it is conceptually impossible to disentangle their relative causal influences. What *is* at stake, however, is the extent to which certain developmental trajectories can be satisfactorily given rich evolutionary explanations and to what extent they are best explained in other terms. This debate pivots on the degree to which learning is conceptualised as being *specifically* directed in nature in a manner commensurate with evolutionary theory.

An extreme nativist position suggests that the role of learning is minimised and consists of simply filling in the slots of richly structured, innate conceptual system. The role of the environment is drastically attenuated and is seen as simply providing a triggering role in the maturation of innate biological processes. In this view the genes direct development which unfolds according to an innately specified design; the environment only provides the necessary support, it plays no active role. At the other extreme, the role of genetic factors are downplayed and learning itself comes to the fore. All learning is conceived of as guided by general rules, with the environment providing the essential forces by which the organism comes to gain an understanding of the world. From this perspective, evolution may provide some general biological endowment, but the environment plays the pivotal role in directing, moulding, and shaping this endowment into myriad phenotypic forms.

It is doubtful whether anyone actually adheres to either of these polar perspectives, although Chomsky and Fodor lean towards extreme nativism, as did some early ethologists, while the extreme empiricist position may be seen in the work of some behaviourists and perhaps those advocating connectionist models of learning (Keil, 1990). There is certainly a large range of alternatives between these two extreme positions, or as Sterelny (1992, p. 102) pithily expresses it: "There are levels of analysis between an appeal to an unspecialised plasticity and the attribution of biological function to bathsong." There is a general agreement that there must be some constraints[1] on learning (e.g., Keil, 1990; Seligman & Hager, 1972; Millikan, 1993; Tooby & Cosmides, 1992). That is, there must be some *a priori* bias on the kinds of information which an organism responds to and the sorts of representations which it constructs about the world. The world can be partitioned in a limitless variety of ways, but only a fraction of these partionings are going to be such as to allow the organism to respond to the world in a manner which is likely to ensure its survival. It is reasonable to suggest, therefore, that learning is typically guided in such a way which is explicable given the evolutionary history of those learning mechanisms and the kinds of environments that they have been embedded in. Constraints therefore are intrinsically interactional in nature. ". . . they are constraints on what sorts of knowledge representations an organism will construct given a range of environments" (Keil, 1990).

This epigenetic view of learning suggests that there can be no partioning of genetic influences from environmental ones. That is, genes and environments are not *separate* causal processes.

> . . . every feature of every phenotype is fully and equally codetermined by the interaction of the organism's genes (embedded in its initial package of zygotic cellular machinery) and its ontogenetic environments - meaning everything else that impinges on it. By changing either the genes or the environment any outcome can be changed, so the interaction of the two is always part of every complete explanation of any human phenomenon. As with all interactions, the

[1] Although Tooby and Cosmides prefers the term 'enablers' rather than constraints.

> product simply cannot be sensibly analysed into separate genetically determined and environmentally determined components or degrees of influence. For this reason, *everything,* from the delicate nuance of Richard Strauss's last performance of Beethoven's Fifth Symphony to the presence of calcium salts in his bones at birth, is totally and to exactly the same extent genetically and environmentally codetermined. "Biology" cannot be segregated off into some traits and not others. (Tooby & Cosmides, 1992, p. 83/84)

This constructivist view of development reaches its most elaborate treatment in the work of the developmental systems theorists (e.g., Oyama, 1985, 1991; Griffiths, 1992; Griffiths & Gray, 1994). The developmental systems approach emphasises that all developmental interactants have comparable theoretical status. Genes are not causally prior to or a primary factor in development; nor is the environment. It follows that an explanation for any phenotypic characteristic must be embedded in an account of the developmental process and not reduced to either genetic or environmental factors. Nature is re-conceptualised, on this account, as the *product* while nature is the *process*.

This view of development is surely correct. However, their still remains the question of explanatory *relevance*. There does still seem to be an asymmetry in the sort of explanation that we deem acceptable for accounting for, say, musical preference on the one hand and eye colour on the other. This difference in preferred causal explanation, can be conceptualised as differences in *main effects* (Sober, 1993a). The developmental conditions for some phenotypes such as eye colour are extremely reliable, so that differences in eye colour are likely to reflect differences in genes. For other traits, varying conditions will lead to quite different results; musical preference is likely to reflect predominately environmental differences. Both of these phenomena, however, are equally co-determined by genes and environment.

For the development of adequate evolutionary explanations of psychological phenomena what is required is not that the trait in question is strongly influenced by genetic factors, but simply that the developmental processes which give rise to the characters in question are stable enough to allow for selection to occur, and hence for adaptation explanations to be forwarded. The biological, the psychological, the social, and the cultural, as Oyama (1991) urges, should not be considered as opposing forces, but instead as different levels of analysis. In developing complete explanations of psychological phenomena, we need to consider a range of different levels of analysis which are appropriate in accounting for the phenomenon in question.

Culture as an alternative explanation

The value of the evolutionary programme in psychology can only be ascertained relative to the value of alternative, competing explanations. While there are few coherent global research programmes in the social sciences, there is a body of literature which draws on the processes of culture, social structure, and social learning as the main causal forces which lie behind the generation of psychological and social phenomena. One pervasive criticism of the evolutionary programme in psychology is that it fails to do adequate justice to the concept of culture as both an alternative causal explanation and as a legitimate source of meaning in the analysis of human behaviour.

Many critics of evolutionary accounts of social behaviour (e.g., Gould, 1978; Sahlins, 1976; Kitcher, 1985, 1988) have argued that culture provides a cogent alternative explanation for patterns of apparently adaptive behaviour. Unlike other animals, in humans functional behaviour, even if universal in nature, may be maintained via patterns of cultural transmission rather than by the processes of natural selection. Individuals, therefore, may be conceptualised as primarily a product of culture rather than biology. While biology is not denied some role in the causal production of behaviour, this role is often characterised as being extremely limited in nature. For example, Sahlins (1976, p. xi) suggests: "Biology, while it is an absolutely necessary condition for culture, is equally and absolutely insufficient: it is completely unable to specify the cultural processes of human behaviour or their variations from one system to another." Evolutionary theory it seems, may provide us with an explanation of *how* cultural learning may be achieved, but it fails to tell us anything meaningful about the *content* of culture (Dennett, 1995).

Culture therefore, is conceptualised as an autonomous or semi-autonomous process which can be effectively decoupled from biological evolution. Sober (1993b, p. 215) illustrates this position clearly:

> Biological selection produced the brain, but the brain has set into motion a powerful process that can counteract the pressures of biological selection. The mind is more than a device for generating the behaviours that biological selection has favoured. It is the basis of a selection process of its own, defined by its own measures of fitness and heritability. Natural selection has given birth to a selection process that has floated free.

Importantly, because this selection process operates by Lamarckian means (Gould, 1980) and hence is many orders of magnitude faster than genetic evolution, it can effectively swamp the influences of biological selection (Dennett, 1995). This is clearly illustrated by the proliferation of seemingly maladaptive behaviour found in many cultures. For example, Barkow (1989) discusses the denial of colostrum to infants in many societies as evidence of a trait which is reliably replicated from generation to generation, but from an evolutionary perspective is highly deleterious in nature. The seemingly bizarre menagerie of behaviour found in different cultures around the world, (and no less in the West), seems to be *prima facie* evidence that the proliferation of specific beliefs and ideas is not yoked in any clear or deterministic fashion to the forces of biological evolution.

It follows from these points that culture provides a potentially more appropriate level of analysis for explanations of human behaviour patterns. As Dennett (1995, p. 340) claims "*What we are* is very much a matter of what culture has made us." By themselves evolutionary explanations are inadequate because they ignore the social and historical embeddedness of human behaviour (Flanagan, 1991), and have avoided discussion of the meaning, role and consequence of contemporary behaviour in social and cultural terms (Cantor, 1990).

These criticisms of the evolutionary programme in psychology and the social sciences can be summarised as follows:

(1) Culture serves as an alternative explanation for patterns of human behaviour.

(2) Culture is an autonomous process decoupled from biological evolution.

(3) Cultural processes, because they operate on vastly faster time scales than biological ones, swamp evolutionary processes.

(4) Culture provides the appropriate context for understanding the meaning of human behaviour.

Therefore:

(5) Culture serves as a different and more apposite level of analysis form which to explain psychological and social phenomena.

This argument suggests that the scope of evolutionary explanations in psychology will be severely limited. This will be the case as long as learning, and especially social learning processes, is conceptualised in very general terms, and if the content of learning is considered to be the primary focus for explanations in psychology and the other social sciences.

Is culture unique to humans?

The concept of culture plays an important role in many explanations in the social sciences. A precise and widely agreed upon definition of culture, however, is hard to come by. Boyd and Richerson (1985, p. 2) define culture as "the transfer of information by behavioural means, most particularly by the process of teaching and imitation." Barkow (1989) argues that this definition is too narrow in scope and is a better characterisation of what might be termed 'proto-culture'. Barkow argues that there are three ideas which are central to the definition of culture. One, culture involves social transmission; two, the transmission consists of information; and three, the information is organised in some systematic manner. Given this definition can we say that other animals, apart from humans, may be said to possess a culture, or engage in cultural learning? This issue is important because criticisms of the evolutionary programme in psychology often make reference to the asymmetry between humans and other animals on certain characteristics where culture is often cited as the relevant difference.

Despite a large body of literature directed at the topic of animal culture (e.g., Tomasello *et al.* 1987; Tomasello, Kruger, & Ratner, 1993; Heyes, 1993; Boesch, 1996; Heyes & Galef, 1996; Wrangham et al, 1994), there is no widespread agreement regarding whether or whether not other animals may be said to possess culture. There are certainly different *behavioural traditions* found in many different animal species. Two well-known examples include the milktop puncturing behaviour of some British tits and the sand-washing behaviour found among some populations of macaque monkeys. The richest source of behavioural traditions, however, can be found among wild populations of chimpanzees. Chimpanzees possess a number of population specific behaviours such as nut-cracking, ant-fishing, leaf-clipping, termite-fishing, and leaf-sponging (Boesch, 1996).

Critics argue that what distinguishes these population specific behaviours found in chimpanzees from fully realised patterns of cultural transmission evident in humans is the *processes* which underlie social learning (Tomasello, *et al.*, 1993; Tomasello, 1994). Tomasello *et al.* (1993) point out that social learning can be achieved in a variety of different ways. For example, learning can be achieved via local or stimulus enhancement where the observer's attention is directed towards the model and the stimulus which is the focus of the model's attention. This is a form of socially facilitated *individual* learning because it is the individual who actually works out how to perform the relevant behaviour. In contrast, behaviour can also be acquired via imitative learning or through teaching. The behaviour here is directly modeled on, or moulded by, the relevant other in the learning situation. Tomasello *et al.* (1993) claim that only humans are capable of imitative learning or direct teaching, and that it is this difference which is responsible for the differences in richness and complexity of behavioural tradition in humans compared to other animals. Several experiments support this conclusion. For example, it was found with controlled studies of food-washing and tool-use in cebus and macaque monkeys that patterns of social learning could be explained entirely in terms of local and stimulus enhancement with no evidence of imitative learning having occurred (Visalberghi & Fragashy, 1990). Experimental studies of chimpanzee-learning seem to support this conclusion. After watching a model obtain a food source with a given method, chimpanzees focused on the goal obtained and the tool used to achieve a similar result, whereas young children tended to directly model the *method* employed (Tomasello, 1994). It is suggested that humans have this capacity for imitative learning, which other animals lack, because humans possess a theory of mind which enables them to engage in perspective taking and hence allows then to learn not just *from* but also *through* other individuals. It is this form of learning, which results in the high-fidelity transmission of cultural traits which gives culture its cumulative nature (Tomasello *et al*, 1993).

Many primatologists on the other hand (e.g., McGrew, 1992; Boesch, 1996) argue that other primates, in particular chimpanzees, *can* be said to possess cultures, and perhaps engage in forms of social learning similar to humans. Boesch (1991), for example, argues that he has seen evidence of teaching among wild chimpanzees. The chimpanzee population that Boesch has studied engages in nut-cracking behaviour. The techniques required to break the nuts are difficult and take some time to acquire via trial and error learning. Boesch claims to have witnessed instances of direct intervention by mothers on their infants learning efforts in a manner which facilitated their performance. Furthermore, Boesch (1996) following Heyes (1993), argues that what is important in producing culture is not the specific mechanisms underlying learning but simply that learning is achieved via some degree of social canalization.

It is clear that there is no widespread agreement on the uniqueness of culture or the mechanisms which are necessary for cultural learning. Certainly there are important differences between human and other animal 'cultures', but it is unclear whether these differences are qualitative or quantitative in nature. What I think does emerge from a comparative study of culture is the importance of elucidating the variety of mechanisms which appear to underlie social learning and the sorts of contexts in which they are likely to emerge. Regardless of whether chimpanzees or other animals may be said to possess cultures, it is clear that in a similar fashion to humans, an elucidation of the behaviour of other primates is likely to involve, in part, the role of local social and behavioural traditions.

Does culture represent a truly alternative explanation?

The idea that culture represents an autonomous, alternative explanation for patterns of human behaviour has been challenged from various sources. Evolutionary psychologists argue that culture does *not* represent a complete or alternative explanation for psychological and social phenomena. Biological and cultural forces, it is argued, are not mutually exclusive, but instead should be conceptualised as being complementary in nature (Tooby & Cosmides, 1992). Culture and biology, therefore, should not be regarded as two separate classes of causes, but as part of a single unified process. As Buss (1995, p. 14) expresses it:

> "Culture," "learning," and "socialisation" do not constitute explanations, let alone alternative explanations to those anchored in evolutionary psychology. Instead, they represent human phenomena that require explanation. The required explanation must have a description of the underlying evolved psychological mechanisms at its core.

Similarly, philosopher Kim Sterelny (1992, p. 164) emphasises the point that culture is not a distinct and parallel process to genetic evolution, but should be conceptualised instead as simply an instance of adaptive plasticity. "Cultural life is the product of a very complex interaction of plastic creature with a richly varied and changing environment. Cultural evolution does not pre-empt appeal to natural selection. Rather, enculturation is a special case of adaptive plasticity."

Culture is certainly an important selective and developmental environment for *Homo sapiens* (Barkow, 1989), but this does not by itself invalidate an evolutionary approach to cultural traits. Just as there is no such thing as a fully functioning fish without water, there cannot be a proper functioning human without culture. Invoking culture as the primary *cause* of human behaviour, however, may be inappropriate. What remains to be explained is culture itself and how different patterns of behaviour become reliably reconstructed under different environmental conditions where social structure represents important slice of the environment.

Explaining individual differences from an evolutionary perspective: Universalism vs. Relativism.

Culture seems to be a powerful explanation for human behaviour, because behaviour does vary *across* cultures, but has strong similarities *within* cultures. On the face of it its seems reasonable to attribute these patterns of similarities and differences to processes of learning from one's social environment. The approach of evolutionary psychologists to the phenomenon of cultural differences is to argue that such differences reflect the operation of universal psychological mechanisms under different environmental conditions (Tooby & Cosmides, 1992; Buss, 1995). Because humans exhibit extreme flexibility in their behavioural repertoire, we should *expect* behavioural differences to emerge under different environmental contexts. "Behavioural variation" Cosmides and Tooby (1987, p. 281) claim " is not an embarrassment to evolutionary theory, it is a prediction of evolutionary theory."

Tooby and Cosmides (1992) argue vigorously for the notion of the 'psychic unity' of humankind. It is claimed that at the level of underlying psychological adaptations humans will exhibit uniformity, regardless of local cultural differences[2]. Individual differences, therefore, primarily reflect different developmental trajectories as a consequence of slight genetic differences and different environmental circumstances; they do not typically reflect discretely different personality types or morphs (Tooby & Cosmides, 1990a). Cultural differences cannot be explained without reference to psychological mechanisms, and an account of how those mechanisms come to provide different behavioural results under different environmental circumstances (Tooby & Cosmides, 1992; Buss, 1995).

An important distinction is drawn by evolutionary psychologists here between the evolved and the manifest. The evolved refers to those mechanisms, developmental trajectories and so forth, shaped by natural selection, while the manifest is the behavioural products or outcomes of those mechanisms (Tooby & Cosmides, 1992). This approach to individual differences is summarised by Tooby & Cosmides (1992, p. 45):

[2] Although Tooby & Cosmides (1992) suggests that there may also be a 'thin film' of population specific or frequency dependent adaptive variation.

> One observes variable manifest psychologies or behaviours between individuals and across cultures and views them as the product of a common, underlying evolved psychology, operating under different circumstances. The mapping between the evolved architecture and manifest behaviour operates according to principles of expression that are specified in the evolved developmental mechanisms and the psychological mechanisms they reliably construct; manifest expressions may differ between individuals when different environmental inputs are operated on by the same procedures to produce different manifest outputs.

A similar point is made by Brown (1991) in his book *Human Universals*. Brown claims that there has been a bias in anthropology and other culture studies in favour of *looking* for differences between different cultures. However, underlying the manifest differences between cultures, Brown argues, are human universals. For example, although cultures around the world differ dramatically in the number of colour words that they have in their lexicon, there are universal patterns of colour discrimination. Regardless of the number of colour words that a culture possesses, members of that culture can see and categorize the same number of colours in the same way as individuals from other cultures. Indeed, as Brown (1991) points out, anthropological research could simply not be carried out without the extended repertoire of similarities that exist between peoples of different cultures. From this perspective, psychological and social phenomena remain stable across different cultures, even though they may vary in their expression, regulation, and importance.

To take a simple example, people in different cultures speak different languages. These differences may be termed cultural differences. However, underlying what appears to be a considerable diversity of phenotypes is a species typical language acquisition device which is universal across cultures. In explaining why people 'speak Japanese' or 'speak English' we need an account of the operation of the psychological mechanisms underlying language acquisition and the specific social context in which they develop. 'Speaking Japanese' therefore is neither solely a cultural trait not a biological one, but one which is the result of a reliable developmental process operating under certain cultural condition. It is a *biocultural* phenomenon.

The example outlined above very neatly reflects the operation of innate psychological processes under varying environmental contexts. In this case there is a clear unity at the evolved level which *explains* the diversity at the manifest level. Other examples are a little more obscure. Consider the phenomenon of ritualised homosexuality in Melanesia (Herdt, 1984). Among many diverse tribes in Melanesia male adolescents go through a period of ritualised homosexual behaviour. These rituals centre on the power of sperm and its role in developing male vigour and strength. Among the Sambia of Papua New Guinea this belief is coupled with idea that female secretions are dangerous and strength sapping to males. As a consequence, young boys must go through an extended period of separation from their mothers and indulge in fellatio to build up their strength for man-hood. Associated with the development of homosexual behaviour is the initiation of nose-bleeds, which are used to expel the evil fluids male boys are subjected to at their mothers breasts. Male homosexuality among the Sambia is embedded in a rich meaning-based context and is surrounded by a farrago of seemingly bizarre practices and beliefs specific to this culture.

No doubt it would be possible in this example to invoke the action of psychological mechanisms underlying sexual and perhaps homosexual behaviour (e.g., see Baker & Bellis, 1995). However, the behaviour which is manifest in this example seems to be only diffusely related to these specific psychological processes and is embedded in a belief-system which is culturally unique. How do evolutionary approaches to culture explain these kinds of examples of cultural practices embodied in the case of ritualised homosexuality?

Explaining culture

Tooby and Cosmides (1992) attempt to explain the diversity of cultural phenomena by invoking a distinction between three different types of culture: meta-culture, evoked culture, and epidemiological culture. Meta-culture refers to the shared culture of human-kind and reflects the operation of mechanisms which are functionally organised to interact with regularities of the environment so as to produce universal human traits. Evoked culture is culture which represents the facultative operation of psychological mechanisms under different environmental conditions. This leads to within-group similarities and between group differences. Finally, epidemiological culture refers to the accumulation of representations from the social environment via social learning mechanisms.

The first two types of culture are clearly wedded to evolutionary processes. Psychological adaptations undermine both the similarity and differences found across cultures. The third type of culture - epidemiological culture - is less clearly influenced by psychological adaptations, but according to Tooby & Cosmides (1992) is not therefore a free-floating process. This is because ultimately what influences the distribution of representations from one generation to the next is their compatibility with our evolved psychological architecture.

Dan Sperber (1996) articulates an epidemiological approach to culture along similar lines. The focus of Sperber's work, as with that of Tooby and Cosmides, is on the psychological mechanisms which underlie cultural learning. To explain culture, Sperber argues, we need to account for why some ideas or beliefs happen to be widely distributed while others are not. Sperber (1996) favours an attraction model of culture, one in which the role of both cognitive processes and the environment are brought to the fore. We should focus, Sperber argues, on the psychological and ecological factors of attraction which influence the distribution of representations.

> The environment determines the survival and composition of the culture-bearing population; it contains all the inputs to the cognitive systems of the members of the population; it determines when, where and by what medium transmission may occur and it imposes constraints on the formation and stability of different types of public productions. The mental organisation of individuals determines which available inputs are processed, how they are processed, and which information guides behaviours that, in turn, modify the environment. (Sperber, 1996, p. 113)

We need rich and detailed accounts, therefore, of both the social and historical circumstances in which an individual is embedded as well as an understanding of the underling evolved psychological mechanisms that humans possess. Cultural diversity, Sperber argues, can be primarily conceptualised as the actual domains of proper domains. Over time the actual domains may become considerably distanced from their proper domains; however, ultimately, mental modules fix the range of cultural content.

The approaches of both Sperber and Tooby and Cosmides emphasise the importance of understanding the evolved psychological processes which underlie learning. They also both point to the importance of explicating the social and cultural context in which the individual develops. A more explicitly co-evolutionary model has been developed in some detail by Boyd and Richerson (1985). The approach of Boyd and Richerson also places considerable importance on the role of psychological processes in explaining culture, but there is also attention paid to the way population-level processes influence the distribution of ideas within and across social groups.

The basic argument forwarded by Boyd and Richerson can be formulated as follows (Richerson & Boyd, 1987):

(1) Cultural variation is shaped by evolved psychological predispositions. Some information, beliefs and the like are more likely to be acquired than others.
(2) Some beliefs are weakly affected by evolved predispositions, therefore other processes are important in cultural evolution.
(3) Such beliefs can affect behaviour.
(4) Properties of culture are important selective environments of humans and explain the nature of many evolved predispositions.

The first point in Boyd and Richerson's argument is similar to the positions adopted by Sperber and Cosmides and Tooby, and indicates one legitimate way in which culture can be given an evolutionary explanation. However, Boyd and Richerson also stress the importance of transmission patterns that are only weakly influenced by *specific* evolved predispositions. They also point out that the profile of beliefs and ideas in a culture can exert an influence on natural selection, for example via culturally specified mating patterns. Just which cultural traits are strongly or weakly influenced by evolved predispositions can only be adjudicated on a case by case basis (Richerson & Boyd, 1987). However, Boyd and Richerson do not advance, as some claim, a decoupling of genetic and cultural evolution, although it seems that some traits may be more directly influenced by evolved biases than others. In their model, Boyd & Richerson suggest three different kinds of biased transmission.

A somewhat different approach to culture, one in which genetic evolution is decoupled from cultural evolution, is that of memetics (e.g., see Dawkins, 1976; Dennett, 1995). Memetics proposes that cultural change is a truly evolutionary process. Cultural traits or 'memes' are the units of information which get transmitted over time via diverse media (books, brains etc.), vary in their nature, and exhibit differential survival rates. Just as genes compete for space in the species genome, so to do memes compete for space in the infosphere of human ideas. Memetics has some attractive aspects to it. It seems to account for the sheer proliferation of seemingly bizarre and irrelevant ideas in human cultures. Ideas proliferate not because they are necessarily useful, let alone reproductively beneficial, but simply because they are good at ensuring their own replication.

However, despite some attractive properties, meme theory, critics argue, possesses some telling differences from genetic evolution - differences which render the analogy between cultural and biological evolution problematic. One problem, highlighted by Sperber (1996), is that there is simply too much slack between descent and similarity; the mutation rate of memes is simply to high. Cultural representations are *not* reliably replicated from one mind to another, but are *transformed* as a result of cognitive processes. Memetics, it is argued, omits the crucial role of psychological factors in the distribution of ideas. Furthermore, as Sterelny (1992) points out, memetics is problematic because, in its current guise, there is no handle on *why* some ideas are preferred over others. Natural selection is not a tautology as some have argued, because we have an independent grasp of what fitness is. Memetics however, as Sterelny argues, fails to evade this problem. Indeed it seems to me that the only way we can float a viable mimetic approach to culture is by including the role of evolved psychological mechanisms which influence the distribution of ideas between and across cultures. Any such approach, of course, cannot sustain the idea that culture is an *alternative* explanation of psychological and social phenomena.

Summary

The main arguments directed against the evolutionary programme in psychology from the perspective of culture cannot be sustained. It is difficult to see how culture can be truly considered as either a completely alternative explanation or an entirely parallel process to the forces of biological evolution. Evolutionary theory, therefore, is likely to play *some* role in the explanation of cultural traits. That is not to deny that much of what is of interest to psychologists has to be understood at the social level. Moreover, it is clear that a rich and detailed account of cultural and historical forces is required as part of many complete explanations of psychological phenomena. I think, however, this serves to emphasis the crucial role that the environment plays in developing satisfactory explanations of any biological phenomena. There still remains, however, a lingering doubt that the kind of phenomenon illustrated by the ritualised homosexuality example, seems to be so far removed from evolved psychological mechanisms, and so clearly embedded in local cultural circumstances as to make evolutionary theory irrelevant in explaining its origin and nature.

This issue highlights a repeated concern regarding the specificity of evolutionary explanations. Evolutionary theory may provide an explanation for a range of capacities that humans posses, but fails to satisfactorily further understanding of the details of human life. A way of reconciling some of these concerns is illustrated in the following chapter in the context of jealousy and is elaborated more fully in chapter ten.

Chapter Nine

Explaining jealousy: A case study

Jealousy is an emotion which can play an important role in the dynamics of human romantic and sexual relationships. This role has been explored in countless films, novels, and plays, notably of course in Shakespeare's searching portrayal of sexual jealousy in *Othello*. In this chapter I will critically examine the various different explanations for jealousy that have been developed. My primary focus here is on the epistemic worth of evolutionary explanations of jealousy in comparison to alternative theoretical accounts. This chapter, then, is an exercise in theory appraisal. That is, I will examine to what extent the evolutionary programme provides a more explanatorily coherent account of the various phenomena associated with jealousy than do other theoretical approaches. As will become clear in my analysis, however, the main task will be in elucidating the *complementary* nature of the various different approaches. As such, this chapter serves as a case study of the way evolutionary explanations in general are likely to figure in attempts to promote an understanding of psychological phenomena.

The choice of jealousy as a topic is somewhat arbitrary in nature and I think that other examples would have equally adequately served my purposes here. However, jealousy in particular and the study of emotion more generally are areas in which detailed evolutionary accounts have been offered by a variety of authors (e.g., Panksepp, 1982; Ekman, 1992; Plutchik, 1990). The psychology of emotion has also been pursued from a range of other perspectives such as cognitive (Lazarus, 1991), social (Harre, 1986) and cultural. The diversity of different approaches to emotion is highlighted by Strongman's (1996) text on the psychology of emotion which discusses over a hundred and fifty different theories of emotion. In my analysis of the nature of jealousy and the sorts of explanations that have and should be developed I will offer some ideas regarding ways of integrating various different theoretical perspectives to provide coherent and unifying explanatory accounts.

I begin this chapter with a discussion of the nature of jealousy and the various different aspects of jealous experiences which need to be explained. My account here is somewhat loose in the sense that I provide a relatively motley collection of things to be explained regarding jealousy rather than detailing specific *phenomena* which have been reliably and repeatedly demonstrated. This in part reflects the relative paucity of research in this area. However, my portrayal of jealousy provides a reasonable characterization of the sorts of things that we need to be able to explain to further our understanding of the nature of jealousy.

This discussion of jealousy is followed by an elaboration of the various different kinds of theories or approaches that have been developed in an attempt to explain the nature of jealousy. Finally, I discuss the ways in which the various different approaches to jealousy relate to one another, with a particular focus on the role that evolutionary explanations have to play in furthering our understanding of jealousy. Some of the ideas regarding the nature of scientific explanations presented in chapter one and two will be employed to clarify just what kinds of explanation are being offered by the various different approaches and how in general our understanding of jealousy is likely to be advanced.

The nature of jealousy

Various authors have offered a variety of different definitions of jealousy. Some approaches lump together romantic and sexual jealousy with sibling jealousy, or with the emotion of envy (e.g., Salothey & Rothman, 1991). I shall confine my analysis, however, to what might be termed romantic/sexual jealousy; that is, jealousy that occurs in the context of a romantic-sexual relationship. Wilson and Daly (1992, p. 42) define this kind of jealousy as "the state of being concerned that ones sexual exclusivity is or might be violated." This definition is similar to that offered by most authors. For example, Bringle and Buunk (1985, p. 42) characterise jealousy as an "aversive emotional reaction that occurs as the result of a partners extradyadic relationship that is real, imagined, or considered likely to occur."

There is some agreement among various different researchers that jealousy does not represent a discrete emotion, but is better considered a blend of other emotions such as anger, fear, and sadness (Sharpsteen, 1991; White & Mullen, 1989). Moreover, jealousy is often characterised as a system rather than a discrete entity, which comprises "particular patterns of emotions, thoughts and actions that emerge in particular social and psychological situations" (White & Mullen, 1989, p.1). In detailing the aspects of jealousy that need to be explained we need to focus on those particular thoughts, feelings and actions which are typically invoked under particular environmental contexts.

General features of jealousy

The first and most important thing that is in need of an explanation is the phenomenon of jealousy itself and why it is reliably experienced in certain contexts but not in others. That is, why do individuals experience jealousy when they perceive a potential threat to their relationship *in the form* of a sexual or emotional rival for their partner's affections? We need to be able to explain here why jealousy is experienced in this context, but not say in the context of a threat to a relationship which is instigated by *other means,* such as partner incompatibility, or geographical separation. We also need to explain the ubiquity of jealousy. That is, why is jealousy reliably experienced by both females and males, by peoples of different cultures and by people at different times in history? That is, why is jealousy such a common problem in close relationships?

We also need to be able to explain the specific phenomenological experience of jealousy. In the appropriate context subjects report experiencing a 'jealous flash', an emotion often described as intense anger or fear (White & Mullen, 1989). Anger appears to be the most common emotion experienced in a jealous episode, followed by fear and sadness (Shaver, Schwartz, Kirson, & O'Conner, 1987). The exact experience, however, depends in part of the specific context. Suspicious jealousy is more often associated with feelings labeled as jealousy and insecurity, whereas confirmed jealousy is typically associated with emotions of anger and sadness (Zammer & Fischer, 1991).

Responses to jealousy

Responses to jealousy-invoking situations may be various, but they seem to fall into certain patterns. Three factors that emerged in a factor analytic study conducted by Buunk (1982) were avoidance, reappraisal and communication. White and Mullen (1989) extended this list to identify eight different coping strategies which include improving the primary relationship, developing commitment, derogating the partner or rival and developing alternatives to the primary relationship.

One important strategy identified by a number of authors (Buss, 1988; Buunk, 1982) was to improve the quality of the primary relationship. Subjects suggested that fulfilling the desires of the other partner, and hence increasing the value of the relationship could achieve this. Tactics that subjects in a study by Buss (1988) suggested would be effective were the provisioning of gifts and kindness by males and the enhancement of appearance by females. More generally speaking, any behaviour which could be seen as fulfilling the preferences held by the partner would be likely to be successful.

Another general strategy is to employ forms of emotional manipulation. For example, the inducement of guilt in a partner might be used to prevent or stop the occurrence of an extra-dyadic relationship. This might be achieved by invoking either the norms of society or the norms, explicitly or implicitly, established in the relationship itself (Buunk, 1991). The deliberate inducement of jealousy is also reported on by a number of researchers (e.g. White & Mullen, 1989) as a strategy to increase commitment from one of the partners or as revenge for a prior infringement of the relationship's exclusivity by one of the partners.

Another kind of response reported by subjects, more appropriate to suspicious jealousy, was to maintain some kind of vigilance on one's partner (Buss, 1988). This sort of vigilance might include calling on the partner at unexpected times, reading their correspondence and spending long periods of time with the partner thereby decreasing their opportunities for extra-dyadic affairs. These kinds of responses are seen in both sexes, although they seem to be more prevalent among men (Buss, 1994). In a study by Flinn (1988) male mate guarding was also shown to correlate with the likely fertility of the partner. Mate guarding seemed to be less prevalent for older partners or partners who were pregnant. The concealment of partners is another method employed to reduce the possibility of extra-dyadic relationships, and might be especially used in contexts where rivals are likely to be present (Buss, 1994).

More severe forms of vigilance and mate-guarding include claustration in India, the veiling of women's bodies in Arabic cultures, guarded harems, foot-binding practiced by some Chinese cultures and the practice of chaperoning (Daly, Wilson & Weghorst, 1982). Other measures employed to prevent the likelihood of extra-dyadic sexual relations include the practices of clitorodectomy and infibulation which are performed on women in many countries throughout north-eastern Africa. These procedures involve the surgical removal of parts of the female genitalia, typically the clitoris, and in infibulation the suturing shut of the labia majora so as to make sexual intercourse impossible[1] (Daly et al., 1982; Hicks, 1993).

Other strategies sometimes employed in response to actual or suspected infidelity, include aggression and violence. At the psychological level this may involve derogation of the partner or the potential rival. Physical violence is also a not infrequent response to sexual jealousy (Daly & Wilson, 1988; Daly et al., 1982). Indeed, male sexual jealousy is the most common cause of all types of violence directed at wives. Jealousy is also the major motive for homicides of spouses and other men (Daly & Wilson, 1988). In research conducted by Buss (1988), 46% of all married men reported that they had threatened an intra-sexual competitor within the last year. Although violence and occasionally homicide is more frequently reported to be carried out by men, it is also a strategy employed by women as well.

Finally, another reasonably common response to infidelity by one's partner is simply to leave the relationship. In a detailed study of 160 sample societies carried out by Betzig (1989), adultery was significantly the most common cause of conjugal dissolution. Infidelity across cultures seems to be widely regarded as an acceptable reason for marriage break-ups.

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[1] Typically young girls are infibulated at puberty and defibualted when they get married. They also may be reinfibulated after the birth of a child or if their husband is going away for some period of time (Hicks,1993)

Modifying variables

A number of different variables have been identified by various authors as modifying the nature and intensity of jealousy responses. Many of these factors are associated with the self-esteem of the individual concerned and their overall amount of relationship dependency. For example, perceived inadequacy as a partner is strongly correlated with the likelihood and intensity of jealous experiences (White, 1981; White & Mullen, 1989). Similarly, highly dependent partners were also more likely to express jealousy and experience increased levels of jealousy (White & Mullen, 1989). Furthermore, subjects with greater expected difficulty in finding partners were also more likely to be jealous and experience more intense jealousy (White, 1981). Conversely, levels of satisfaction and commitment were negatively associated with the degree of jealousy experienced.

The qualities of the rival also play an important role in moderating the degree and likelihood of sexual jealousy. More jealousy is invoked, for example, when the rival is perceived as being similar to the partner (White, 1981). Furthermore, when the rival is perceived as being superior to the self on attributes valued by the individual, more jealousy is also experienced (Salothey & Rothman, 1991).

Aspects of the relationship also play an important role in moderating the nature of the jealousy experience. For example, expectations of sexual exclusivity are strongly related to jealousy (White & Mullen, 1989). Jealousy is also more intense when individuals do not have an extra-dyadic relationship of their own at that time (Buunk, 1991).

Gender differences

Many of the variables and strategies identified above have been demonstrated to have fairly reliable gender differences. It is firstly worth noting that there are *no* reliable gender differences in the degree and intensity of jealousy experienced. Both men and women reliably report experiencing jealousy in the context of a threat to the primary relationship imposed by a potential or actual rival (White & Mullen, 1989). However, men and women seem to be more sensitive to different kinds of threat to the primary relationship. Men report experiencing greater distress to potential sexual infidelity on their partner's behalf. Women on the other hand, report experiencing greater distress to emotional rather than sexual infidelity (Buss, Larsen, Westen, & Semmelroth, 1992; Buunk & Hupka, 1987). Physiological responses such as heart rate and electrodermal activity also follow a similar pattern. Men experience more physical distress to the sexual rather than the emotional infidelity scenarios, whereas women experience greater levels of physiological response to the emotional rather than the sexual infidelity scenarios. The basic pattern of these results, although with somewhat different magnitudes, has also been replicated in samples form Germany and the Netherlands (Buunk, Angleitner, Oubaid, Buss, 1996), and from China (Geary, Rumsey, Bow-Thomas, Hoard, 1996). Although this finding is a robust one, it also should be noted that men and women differ in their expectations that casual sex is likely to lead to emotional attachment. Harris and Christenfeld (1996) demonstrate that women are more likely to believe of men than men are of women that casual sex is not going to lead to an emotional attachment with the opposite sex.

In general there seems to be a reasonably robust gender difference regarding the importance of sex in the context of a possible extra-dyadic relationship. Men more than women, for example, emphasize chastity as an important attribute in a mate and see promiscuity as an especially undesirable property of a potential partner (Buss & Barnes, 1986). Indeed, in a study by Buss and Schmitt (1993), men rated faithfulness and sexual loyalty as the most important traits that they look for in a partner for a committed long-term relationship. Unfaithfulness also emerged as the most upsetting behavior that a woman can inflict on a man in a committed relationship. Unfaithfulness is also ranked as distressful for females, but not quite so highly (Buss, 1989).

Males also experience greater threat to their primary relationship when a rival is perceived as superior in terms of sexual behavior. Females on the other hand experience greater threat when the rival is perceived as being more physically attractive and superior in social skills than they are (White & Mullen, 1989). Related to this finding, males but not females were more jealous if an extra-dyadic relationship on the part of their partner led to sexual deterioration in the primary relationship (White & Mullen, 1989). In general, expectations of sexual exclusivity are better predictors of male rather than female jealousy.

The coping strategies employed by females and males also tend to differ to some extent. Males are more likely to employ violence or extreme vigilance tactics than are females and are also more likely to end a relationship or consider an alternative relationship if their partner is engaged in an extra-dyadic affair. Women on the other hand, are more likely to engage in activities aimed at reconciliation and strategies designed to further increase the emotional commitment in the primary relationship. These differences are also seen in patterns of divorce in different cultures. In twenty-five societies, out of a sample of 160, divorce follows from adultery by either partner, in fifty-four societies it follows from adultery on the wife's part and in two societies it follows only from adultery on the males part (Betzig, 1989).

Cross-cultural aspects of jealousy

Jealousy appears to be a cross-cultural universal. There are virtually no reports in the ethnographic literature which indicate that there are any jealous-free societies (White & Mullen, 1989; Buss, 1994). There is also some evidence to suggest that the intensity of emotions experienced in different cultures is fairly similar (e.g., Zammer & Fischer, 1991). However, there does seem to be some variation in the kinds of situations that elicit jealousy and in the nature of the subsequent responses to jealousy-evoking situations in different cultures. For example, among the Pawnee of the American plains requests by another male for a cup of water from one's wife is a sign that the other man is after the wife, whereas in the Saora of India a husband must actually witness his wife engaged in sexual congress with another male before she can be accused of jealousy (White & Mullen, 1989).

Typical coping strategies also vary in different cultures and will depend in part on the laws, customs and norms of a given society. There is, however, a widespread acceptance in many cultures that homicide is a justified response to partner infidelity, especially on a males part (Daly, Wilson & Weghorst, 1982). The severity of aggressiveness by men in jealousy situations, however, seems to be determined in part by the importance attached to marriage in the society, restrictions to extra-marital sex, and the relative emphasis placed on private property (Hupka, 1991).

There is also some evidence to suggest that there are cultures where sexual infidelity is condoned or even encouraged in some contexts (Ford & Beach, 1952). In some Innuit groups, for example, mutual agreement may be reached between two families to exchange mates. Altogether Ford & Beach (1952) identified seven out of one hundred and thirty nine societies where normal prohibitions against extra-marital intercourse are relaxed. However, as Daly et al. (1982) point out, despite the relative sexual permissiveness of these cultures, jealousy is not absent and violent reactions to sexual infidelity are not uncommon. Indeed, jealousy is the leading cause of spousal homicide in Innuit societies (Daly et al., 1982).

Jealousy in open marriages

It would seem from the cultural evidence that sexual permissiveness in a society does not necessarily result in the absence of jealousy. The same seems to be true of open marriages or swinging couples. In a study by Buunk (1981), 80% of individuals in open marriages experienced some jealousy. If the various ground-rules set by swinging couples are strictly adhered to, however, jealousy can be reduced (Buunk, 1991). However, despite a strong self-selection bias for lack of jealousy, jealousy seems to a reasonably common problem experienced in open marriages and swinging couples. As White and Mullen (1989, p. 122) conclude: "despite [considerable] efforts up to a third of swingers soon drop out because of their own or their partner's jealousy."

Legal and social structures

Many of the laws that exist or have existed regarding sexual infidelity reflect to a considerable degree the features of jealousy illustrated in the foregoing discussion. For example, many laws cross-culturally and historically reflect male concerns over sexual infidelity and a double standard with respect to the morality of adulterous behaviour. There are many laws in force, for example, which exonerate a man for killing his adulterous wife or wife's lover. In Texas until 1974 it was legal for a husband to kill his wife and her lover if he found them engaged in sexual intercourse (Daly & Wilson, 1988). Cross-culturally, adultery is often viewed as a mitigating circumstance in the context of homicide. Many of the laws relating to adultery seem to reflect general laws regarding property rights and adultery is usually defined in terms of the marital status of women (Wilson & Daly, 1992).

An evolutionary approach to jealousy

From an evolutionary perspective jealousy is conceived of as a system of feelings, thoughts, and behaviours, which have evolved because of the reproductive advantages that they conferred on our ancestors. That is, those individuals, both males and females, who experienced jealousy under certain narrowly delineated circumstances and acted in various sorts of ways were more likely to be reproductively successful than those individuals that did not experience jealousy. Jealousy, therefore, is hypothesized to be an adaptation: a trait with a proper function under normal conditions (Daly et al., 1982; Wilson & Daly, 1992; Buss, 1995).

Explanations of jealousy from an evolutionary perspective draw on both the hard core of the evolutionary programme, as noted above, and the conceptual resources of two important auxiliary theories – parental investment theory and sexual selection theory. Parental investment theory (Trivers, 1972) suggests that in any species with internal fertilisation there will be an asymmetry between the two sexes with respect to their confidence regarding the certainty that any offspring will be their own and not somebody elses. Females in most species can be assured that the offspring that they raise are their own; typically males are rather less certain. This uncertainty of paternity represents a strong selection pressure operating against extended paternal investment. Because a male can never be sure that the offspring that he is investing in is his own, the benefits of investing time and resources into raising offspring are likely to outweighed by the potential costs of being cuckolded.

In species where parental investment *has* evolved, we would expect the evolution of various strategies that increase the likelihood of paternity certainty. That is, there would be strong selection pressures for the evolution of any mechanisms which decreased the likelihood that the investing male would raise and provision offspring that were not his own. Similarly, to the extent that paternal investment is an important component for the viability of offspring, we would expect the evolution of various mechanisms in females which decreased the likelihood that their mate would desert them or divert resources to other females. The strength of the selection pressures here will be determined, to a large extent, by the likelihood that either sex will indulge in sexual activity outside of the primary mateship. The evolution of mechanisms designed to decrease paternity uncertainty and increase the confidence of paternal investment will be a function, in part, of the sexual proclivities of both sexes. Furthermore, sexual selection theory suggests that both females and males would benefit by selecting partners which are most likely to further their evolutionary goals in terms of assuring paternity certainty and assuring continued paternal investment in offspring.

Evolutionary approaches suggest that the mechanism that has evolved in humans to decrease paternity uncertainty and increase confidence in paternal investment is the psychological/motivational/emotional complex which we label jealousy (Daly et al., 1982; Wilson & Daly, 1992; Buss, 1994).

> Ancestral hominids needed a psychological mechanism specifically designed to alert them to potential threats from the outside, a mechanism that would regulate when to swing into action in deploying mate guarding strategies. That mechanism is sexual jealousy.
> (Buss, 1994, p. 125)

Those who promote an evolutionary approach to jealousy suggest that the evolutionary research programme offers a coherent and unified way of understanding the various factors associated with jealousy. The emotion of jealousy exists because it represents an adaptive solution to the problems associated with paternity confidence and assuring paternal investment. Jealousy only, or at least predominately, occurs in the context of threats to the primary relationship brought about by actual or possible extra-dyadic relationships on the part of one of the partners because it is just *this* kind of threat which may possibly lead to a drastic decrease in reproductive success. The break-up of a pair-bond for other reasons, although detrimental in terms of wasted time and resources, does not represent quite such a threat. This is because the occurrence of extra-dyadic relationships threaten to totally undermine the investment of the male as he may end up provisioning children which are not his own. For women the possibility of an extra-dyadic relationship on the part of her partner threatens the flow of key resources potentially essential for the survival of her offspring.

The adverse phenomenological nature of jealousy can be explained in terms of general evolutionary approaches to emotion. The affective nature of emotion exists in order to motivate individuals to behave in certain ways that are adaptive. The pain of jealousy has evolved as a means of promoting various ways of acting that relieve that pain and which generally are likely to lead to successful reproductive outcomes.

The typical patterns of thoughts and actions associated with jealousy are explained from an evolutionary perspective as ways of either preventing extra-dyadic relationships (mate-guarding, vigilance, etc.), preventing the break-up of a relationship (investing more resources, derogating the rival, using emotional manipulation), signaling to the partner and relevant others the unacceptability of the extra-dyadic behaviour (violence, homicide), or simply cutting one's losses (leaving the relationship). The evolutionary approach argues that jealousy is a facultative adaptation which can lead to a variety of different strategies depending of the context of the situation and the nature of the relationship.

Explanations of jealousy drawn from the evolutionary research programme can also further our understanding of the range of variables associated with jealousy. The relationships between jealousy, self-esteem, relationship dependence and the quality of the potential rival can be understood in terms of the relative importance of the specific relationship to the individual and the likelihood that the relationship will be subverted by a relevant other. In the context of an extra-dyadic threat, low-quality mates who are highly dependent on the current relationship are likely to suffer greater reproductive losses than high-quality mates who may be able to easily find another mate. If the rival is superior to oneself and is similar to the partner in their attributes, then the chances that the relationship will be undermined will be considerably increased.

The various gender differences found in jealousy have also figured prominently in evolutionary accounts. The pervasive gender difference in the salience of sexual vs. emotional infidelity is explained in terms of the different reproductive threats faced by men and women. For men, a single sexual encounter by one's partner[2] may result in the costs of rearing offspring that are not one's own. This potentially represents a major fitness cost. Hence, this explains why men seem especially concerned with sexual infidelity and why chastity but not promiscuity are valued traits in a mate. This also explains why jealousy is greater when the extra-dyadic relationship is related to a decrease in sexual activity in the primary relationship and why jealousy is sometimes associated with sexual arousal. From the male's perspective, even given the presence of a significant other, current and continued sexual activity with one's mate is likely to be adaptive. For women, the major fitness cost resulting in one's partner's extra-dyadic sexual relationships is the divergence of resources from the current mateship to the new one. This explains why emotional infidelity is more salient to women, as it is a more reliable indicator that her mate is likely to abandon her or to decrease his level of paternal investment. Or course both emotional and sexual infidelities are of concern to both males and females because they are likely to be highly, but not perfectly, correlated with one another.

The difference between men and women in coping strategies can also be explained from an evolutionary perspective. Because the costs of sexual infidelity are greater for males than for females, the use of vigilance, violence, and desertion represent strategies designed to prevent extra-pair copulations and to immediately withdraw resources if extra-dyadic sexual relations are discovered. The prevalence of relationship improving tactics on the part of females can be explained in terms of the importance for women in maintaining the flow of resources that are obtained in the primary relationship even in the face of some sexual infidelity.

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[2] Under historical conditions of course, the lack of reliable contraception would make this a real possibility.

The ubiquity of jealousy across cultures is explained by the evolutionary perspective in terms of the presence of universal psychological adaptations. The fact that jealousy occurs in all societies and even in the context of marriage situations which sanction extra-pair relations, is explained in terms of the developmentally robust construction of those processes that underlie jealousy and is testimony to the reproductive advantages that accrue to those individuals who possess those mechanisms. Differences in responses between different individuals in different cultures and the variety of different specific strategies which are employed by individuals in various cultures are not *directly* explained from an evolutionary perspective, even though it is argued that evolved psychological mechanisms are at the heart of any explanation of these differences. Specific phenomena such as infibulation are only partly explicable in terms of men's evolved psychological mechanisms, and the relevant details of the social structure, and the beliefs of the community also need to be invoked to advance a complete understanding of this phenomenon.

The prevalence of certain laws relating to adultery has also been explained from an evolutionary perspective as the formal instantiation of male's concerns regarding sexual infidelity. Because in most societies males have political power, many of the laws will reflect the interests and concerns of men. The legal formalization of the double standard, therefore, is just a manifestation of male's sexual proprietriness and the chronic male fear of cuckoldry. Evolutionary theory only offers part of the explanation here, however, because specific laws and changes in the law are also caused be various social mechanisms not necessarily related to the evolved psychological preferences of men.

The evolutionary approach to jealousy, therefore, seems to offer a coherent and unified approach to explaining many of the phenomena associated with jealousy. Questions, however, have been raised regarding the viability of the evolutionary approach. Just what evidence is there to suggest that jealousy really is an evolutionary adaptation? The first step in developing a viable evolutionary explanation for jealousy is to establish that the conditions under which humans evolved were such, given parental investment and sexual selection theory, that we would expect a mechanism like jealousy to be selected for.

Contemporary humans are characterized by internal fertilization and gestation, pair-bonding with considerable male investment, and a tendency towards polygyny. Moreover, in humans female ovulation is concealed, therefore increasing the window of potentially relevant sexual infidelity to which human males should be sensitive. Importantly it is worth noting that pair-bonding, socially sanctioned in marriage, is a human universal. Furthermore, although the amount of contribution will vary individually and cross-culturally, males often invest heavily in terms of time and resources in the raising of offspring. Contemporary studies of extra-dyadic relationships also suggest that infidelity is a not too infrequent occurrence for both males and females (see Baker & Bellis, 1995). Pair-bonding, parental investment and sexual infidelity certainly characterize contemporary human populations, but what evidence is there to suggest these features were present in our evolutionary past when the putative mechanisms underlying sexual jealousy were selected for?

Several lines of evidence suggest that pair-bonding between males and females with reasonably high levels of parental investment may have characterized the social structure of early hominids. The evolution of pair-bonding in *Homo* has been linked to bipedalism, increase in brain size, the shift to a predominantly terrestrial environment and the increasing dependency on meat as a primary food source (e.g., Lovejoy, 1981; Foley, 1987; Foley & Lee, 1989). In the increasingly dry and mosaic environment characteristic of the Pleistocene two million years ago, it is suggested that early *Homo* began to rely more heavily on meat, either scavenged or hunted, as an important source of food. A consequence of meat eating, given the relative patchiness of resources, is the increase in home range and day range size. About two million years ago a rapid increase in brain size is also observed. Given the biomechanics of giving birth to a large-brained infant from a body adapted to bidepalism, a relatively premature birth would have been the only solution. Indeed, for a mammal of our brain size a pregnancy of 21 months would normally be expected. The premature nature of *Homo* infants means that they required extended periods of care and provisioning.

Increasing maternal reproductive costs associated with infant vulnerability and the wider home range associated with meat consumption would have led to the increase in the frequency and intensity of male-female associations (Foley & Lee, 1989). One solution to these problems would have been the establishment of stable pair-bonds between males and females.[3] The advantage to females is the increased viability of their offspring through male provisioning of meat. The presence of such bonds would also have been advantageous to men in that they were capable of monopolizing a single female. This may have been especially important given the evolution of concealed ovulation in females and hence the uncertainly for men regarding optimal times for copulation. The low levels of sexual dimorphism characteristic of the *Homo* line also suggest the likelihood of some form of pair-bonding. The relative difference in size between males and females in a good indicator, across species, of the degree of polygamy found in that species. Large differences between males and females, as found say in elephant seals, is associated with high level of intra-sex competition and a strongly polygamous mating strategy. The relatively slight sexual dimorphism found in *Homo*, is indicative of monogamy or mild levels of polygamy. The available evidence, therefore, seems to suggest that the pair-bond, ubiquitous in contemporary human populations, was a feature of our ancestors for some two million years or so.

There is also evidence to indicate that sexual infidelity in the form of multiple matings was a feature of Pleistocene sexual behavior for both males and females. Firstly, it is worth noting the potential reproductive benefits that both males and females might have accrued by pursing a strategy which involved some degree of sexual infidelity. For males, unburdened with the costs of gestation, lactation and so forth, any additional copulations would represent a low-cost boost to their overall reproductive success. Therefore, we would expect fairly strong selection pressures in favour of male sexual promiscuity. However, the extent of male sexual promiscuity will be determined, in part, by the preferences of females. That is, male promiscuity could not have been selected for if historically females had denied males the expression of that trait (Smith, 1984). The advantages for female sexual promiscuity may seem less obvious. After all, a female's reproductive success is limited to the number of offspring that she can sire and cannot be considerably increased through additional sexual couplings.

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[3] An alternative strategy would have been to increase female kin-bonding and the use of other females as providers. Foley and Lee (1989) argue, however, that given the demographic and ecological constraints characteristic of early *Homo* populations this would have been difficult to establish.

A number of benefits, however, have been suggested for female promiscuity (Smith, 1984; Baker & Bellis, 1995). Firstly and most importantly, it has been suggested that a strategy of multiple matings employed by females is one way of obtaining 'good genes'. Some females may have to settle for relatively inferior males in their primary mateship. A mixed strategy of adopting a façade of monogamy while obtaining resources from a primary mate, and pursing copulations with available superior mates would have proved a reproductively beneficial option (Smith, 1984). Following Hamilton and Zuk (1982), it is suggested that females seek partners who exhibit indicators of pathogen resistance. To the extent that their primary partner does not possess such indicators, females may benefit from pursuing copulations with healthier males. Some empirical support for this idea is offered by a variety of studies (e.g., Gangestad & Simpson, 1990; Gangestad & Thornhill, 1987) which suggest that males who possess indicators of pathogen resistance[4] are judged as more attractive by females and have relatively more sexual partners than those men who do not posses such indicators.

Other potential advantages to females who pursue a covert polyandrous strategy include the accumulation of material resources, physical protection, and fertility back-ups. Multiple matings can also be viewed as a means of increasing the genetic diversity of offspring (Smith, 1984).

Both human males and females possess a number of physiological attributes, which suggest a long history characterized by casual sex, infidelity, and multiple matings. Sperm competition theory (Smith, 1984; Parker, 1982; Baker & Bellis, 1995) suggests that in males the size of the testis, the number and type of sperm, and the size and shape of the genitalia provide strong clues as to the degree of sperm competition or double-matings that a species has experienced in its evolutionary past.

In primates the size of the testis, which are positively correlated with overall sperm production, are strongly associated with the kind of mating system that is found

[4] One important indicator, it is suggested here, is the level of fluctuating asymmetry. Fluctuating asymmetry, or the degree of asymmetry of bilateral characters, appears to be heritable and indicates the relative levels of developmental perturbation. The level of fluctuating asymmetry, therefore, increases with exposure to environmental factors such as pathogens.

(Harcourt, Harvey, Larsen and Short, 1981; Martin & May, 1981). Among the hominoids, gorillas and orangutans have the smallest testis relative to body size and hence the lowest overall production of sperm. Both the mating systems of gorillas and orangutans are characterized by male monopolization of females either in the context of harems, as in gorillas, or in terms of defending a portion of territory, as in orangutans. Double matings are extremely rare occurrences in both gorilla and orangutan societies, hence there is little selective pressure for increase in sperm production. Chimpanzees by contrast have the largest testis relative to body weight of any hominoid. The chimpanzee has a multi-mating system in which an estrous female is sequentially mated by a large number of males over a short period of time. Consequently there is a strong selection pressure for an increase in sperm production. Human males have testis somewhere intermediate in size between chimps and gorillas; this suggests that our evolutionary history has been characterised by a moderate degree of sperm competition and a reasonable amount of double-matings.

The *kind* of sperm that humans produce further supports the idea that double-matings were prevalent in our evolutionary past. Recent work by Baker and Bellis (1995) indicates that only a relatively small percentage of sperm are actually programmed as egg-getters. Most of the sperm produced by males seem to function in the context of sperm-competition by either acting as 'blockers' preventing the movement of alien sperm, or as 'kamikaze sperm' actively seeking out and demobilizing the sperm of other males. Furthermore, Baker and Bellis (1989, 1995) provide some evidence to suggest that the total production of sperm in an ejaculate is adjusted depending on the relative likelihood that the male's partner had engaged in an extra-pair copulation in the recent past.

Human females also possess a number of physiological mechanisms which indicate a history of casual mating and extra-pair copulations. The evolution of concealed ovulation and continuous sexual receptivity can be viewed as a way of concealing current reproductive value and hence encouraging continual material support from the primary partner, as well as enhancing opportunities for extra-dyadic sexual relations (Smith, 1984). The finding that females are more likely to engage in extra-pair copulations when they are ovulating and that their patterns of orgasm are differentially geared to increase the likelihood of conception from males outside of the primary pair bond, provides further support for the idea that casual matings were likely to be a prominent feature of our evolutionary history.

The foregoing discussion suggests that the conditions whereby jealousy might have evolved were likely to have been a prominent feature of our evolutionary past. This adds plausibility to the claim that jealousy is a psychological adaptation which has evolved to solve the problems posed by uncertainty over paternity and paternal investment. Evolutionary approaches provide further support for the idea that jealousy is an adaptation by drawing on comparative studies of other species (e.g. Buss, 1994; Wilson & Daly, 1992). In other words, analogical reasoning is employed to further the explanatory coherence of evolutionary approaches to jealousy. That is, if it can be shown that other animals possess similar strategies to those possessed by humans then the claim that jealousy-related mechanisms in humans are adaptations is strengthened.

The absence of parental investment in other primates and mammals generally suggests that the most fruitful source of analogy for human jealousy might be drawn from the avian order (Wilson & Daly, 1992). In a series of studies Anders Moller (Moller, 1987, 1988) has demonstrated a number of attributes found in swallows related to the risk of sexual infidelity which are seemingly analogous to those found in humans. For example, swallows engage in active mate-guarding of their partner but only while their mate is fertile. Males who possess attributes preferred by females and which may be possible indicators of pathogen resistance are also more likely to achieve extra-pair copulations and less likely to be cuckolded themselves. Male parental effort also seems to be adjusted relative to the likelihood that the male's partner had engaged in extra-pair copulations during her fertile period. That is, male parental care is positively correlated with the number of in-pair copulations that the male and her partner had engaged in and negatively correlated with the number of extra-pair copulations that the male's mate had experienced.

Similar patterns of sexual proprietariness are also observed in a number of other species of birds, such as dunnocks (Davies, 1986), as well as in a number of different insect species (Thornhill & Alcock, 1983). Many of these species also posses a variety of mechanisms relating to sperm competition such as the use of copulatory plugs, sperm scrapers, and the adjustment of ejaculate size and type (Baker & Bellis, 1995).

In developing evolutionary explanations of human jealousy these comparisons with other species are actively drawn upon. For example, Wilson and Daly (1992, p. 297) argue that the studies carried out on birds "provide strong evidence that paternity investing male animals have evolved sexual psychologies designed by selection to reduce both the likelihood of cuckoldry and its costs once incurred." and conclude that, "we may expect no less of the evolved psyches of paternally investing *Homo sapiens*." Similarly, Buss (1994, p. 125) argues that: " Although the phylogenetic distance between humans and insects is vast, the basic adaptive logic behind holding onto a mate shows striking parallels. Males in both cases strive to inseminate females and to prevent cuckoldry. Females in both cases strive to secure investments in return for sexual access." However, "human tactics to retain a mate take on uniquely intricate forms of psychological manipulation . . ."

Comparative explanations are used, therefore, as a means of furthering the claim that jealousy represents an evolved adaptation designed to solve a specific adaptive problem in the environments which humans inhabit and have inhabited throughout the course of the Pleistocene. The evolutionary research programme provides an explanation for jealousy in humans which draws on the general resources of the programme and which seeks to explain a diverse range of physiological, psychological and behavioral characteristics of a variety of different species. These characteristics are explained in terms of the problems of assuring paternity certainty and paternal investment under specific ecological conditions that involve pair-bonding, paternal investment and extra-pair matings.

Criticisms of evolutionary approaches to jealousy

The evolutionary approach to jealousy has received a range of different criticisms from a variety of authors (e.g., White & Mullen, 1989; Hupka, 1991; Desteno & Salovey, 1994). Typically speaking, these criticisms reflect some of the general criticisms of evolutionary explanations in psychology discussed in chapters four to eight. Critiques of evolutionary approaches to jealousy, therefore, can be viewed as challenges to the overall explanatory coherence of the evolutionary research programme in its attempts to provide satisfactory explanations of the various phenomena related to jealousy.

Many of the criticisms of evolutionary approaches to jealousy are directed towards the idea that jealousy can be reasonably considered as an adaptation with a distinct evolutionary history. These criticisms suggest that evolutionary explanations of jealousy are based on inadequate evidence, weak methodology, and inappropriate generalisations from other species (e.g., White & Mullen, 1989; DeSteno & Salovey,1994). For example, White and Mullen (1989) claim that evolutionary psychologists adopt what they terms as the 'Pleistocene fallacy', assuming the existence of jealousy in our common ancestors on the basis of little or no evidence. Jealousy obviously leaves no fossils, so how can we know that it truly represents an adaptation to Pleistocene conditions? The answer here of course is that we cannot know with absolute *certainty* that jealousy was a feature of hominid social behavior during the Pleistocene. However, as I have elaborated upon at various points in this thesis, the unobservable status of entities should be no barrier to their use in the development of adequate explanatory theories. Moreover, there are various converging lines of evidence to suggest, as I have discussed in some detail above, that the conditions for the evolution of jealousy were present throughout the evolutionary history of our ancestors.

The view that evolutionary explanations of jealousy are vague, disconfirmable and based on after-the-fact reasoning have also been forwarded (White & Mullen, 1989; DeSteno & Salovey, 1994). White and Mullen suggest, for example, that the use of after-the-fact and analogous reasoning are inferior forms of argument which they consider to be 'pre-scientific' in nature. These comments, as I have argued in other contexts earlier, reflect a misunderstanding about the nature of science and what constitutes good scientific practice. A realist approach suggests that science is primarily in the business of elucidating and explaining phenomena. It is a reasonable practice, therefore, to develop explanations for events in the world *after* clarification of just what those events are. The use of analogy in developing explanations is also a legitimate form of scientific reasoning. Indeed, as Thagard (1992) has illustrated, the development of the theory of natural selection by Darwin (among other examples) drew heavily on the use of analogies between artificial and natural selection. The use of comparative explanations in evolutionary accounts of jealousy, therefore, is perfectly legitimate. However, as I note in chapter five, we should be careful to avoid facile comparisons which do not pay due attention to the causal processes underlying the traits of comparison.

The criticism that evolutionary explanations of jealousy are unfalsifiable is also a common but mistaken one. Certainly no *one* experimental result will lead to a rejection of evolutionary theory or even parental investment theory, but for what it is worth, the specific hypotheses offered by evolutionary approaches are disconfirmable in pretty much the same way as any specific hypotheses developed from any theoretical approach. The claim by Buss et al. (1992) that there should be a gender difference in the kinds of situations that elicit the greatest amounts of jealousy, for example, could have been clearly shown to be inconsistent with the subsequent experimental results.

Critics of evolutionary accounts have also pointed to various apparent anomalies in the jealousy literature which they argue cannot be explained adequately from an evolutionary perspective. For example, how ask White and Mullen (1989), can murdering one's spouse in a fit of jealousy possibly be reproductively advantageous? Evolutionary psychologists have offered various different explanations for this apparent anomaly. Daly and Wilson (1988) argue that homicide is simply a slip in a mechanism that was designed to promote anger and aggression under certain circumstances, but not necessarily murder. Buss (1994) however, suggests that homicide might well be adaptive in the sense of increasing relative reproductive success, and by issuing a clear message to future potential partners. Whether or not either of these suggestions is correct remains an open question. However, it is also worth noting that it is not clear how the *other* approaches to jealousy, that I will outline shortly, adequately explain why homicide is a potential response to a jealousy evoking situation.

Other kinds of criticism directed at evolutionary approaches to jealousy suggest that they fail to take into account the plasticity of humans and the role of culture and social structure in determining behavior (e.g., Hupka, 1991; White & Mullen, 1989). For example Hupka (1991, p. 254) suggest that:

> It appears reasonable to propose that human beings have evolved the capacity to be emotional; however, in light of their high levels of intelligence, immense investment of time in the care of offspring, and long periods of learning, it is also reasonable to propose that all other facets of emotion (e.g., their elicitation, expression, modulation, the target of the emotion, etc.) are learned.

Similarly, DeSteno and Salovey (1994) claim that it is impossible to prove that genetic factors, as opposed to other social and psychological variables, represent the true causes of jealousy. These kinds of criticisms, I believe, confuse the way that we should understand the role of different causes in development. As I have elaborated in chapter eight, for any species with some degree of phenotypic plasticity, traits are best conceived of as being constructed during ontogeny due to the influences of multiple, interactive causes. What is required for an evolutionary explanation of jealousy is not to demonstrate that it is genetically determined, but simply to show that jealousy is a reliable developmental outcome under specific environmental contexts. Hupka's (1991) claim that jealousy is a social construction is certainly true in some important senses. Jealousy would not occur outside of the specific social arrangements that characterize human societies which include exclusive relationships between two individuals. For the evolutionary psychologist, however, these facts simply represent the selective environments that have characterized both contemporary and ancestral human societies in which the emotion of jealousy has evolved. In other words, exclusive pair-bonds represent the *Normal* conditions in which it is the *proper* function of jealousy to be activated. Evolutionary psychologists view the variability in the expression of jealousy across cultures as the result of slightly different developmental inputs into the universal psychological mechanisms underlying jealous experiences.

It is certainly the case that evolutionary accounts of jealousy fall short of an ideally complete adaptation explanation; however, they are also far removed from the realm of the highly speculative 'just-so' stories which have been offered for some traits. The idea that jealousy in humans is an adaptation with an evolutionary history, therefore, should be considered to be an epistemically plausible suggestion and one that warrants further consideration. Ultimately, the value that we place on evolutionary approaches to jealousy will be determined by the extent that they increase our understanding of the various phenomena related to jealousy. That is, we should prefer evolutionary explanations to alternative accounts if they provide more coherent explanations of the various aspects of jealousy.

Cognitive approaches to jealousy

Various authors have offered explanations of jealousy from a broad-based cognitive perspective. Drawing on the work of Lazarus (1991), Mathes (1991), White and Mullen (1989) and Parrott (1991) all articulate theories of jealousy which follow from a cognitive appraisal perspective on emotion. This sort of approach focuses on the appraisal processes which individuals undergo in certain contexts. That is, the focus is on the evaluation of events in terms of their relevance and significance to an individual's life and the goals that they may possess.

The cognitive model offered by Mathes (1991) proposes three major stages in the experience of jealousy: primary appraisal, secondary appraisal and reappraisal. During primary appraisal an individual evaluates an event for its significance to that person's well being. Events can either be appraised as irrelevant, benign/positive, or stressful. Secondary appraisal involves the evaluation of various possible courses of action based on the primary appraisal of the situation experienced. During reappraisal the situation is reevaluated depending on changes in the relevant circumstances brought about by coping, further reflection and so forth.

Thus, when an individual is confronted with an actual or potential love triangle, they engage in primary appraisal of the situation. If the situation is likely to lead to loss of resources valued by the individual or the thwarting of important goals, then it will be appraised as stressful in nature and the individual will experience jealousy. During secondary appraisal the individual determines what kind of strategies to employ to cope with the situation. These may involve the use of physical force, emotional manipulation and so forth. Just what strategies are employed may be determined by a variety of factors including the details of the situation encountered, the values of the individual concerned and aspects of the individual's personality. Secondary appraisal is followed by the implementation of the various coping strategies and by a reappraisal of the situation (Matthes, 1991).

White and Mullen's (1989) model is similar in many respects to the one presented by Mathes (1991). The focus as with Mathes, is on the various stages of appraisal and the implementation of coping strategies of either a cognitive or behavioral nature. White and Mullen also emphasis in their model the role of self-esteem in the evaluation process, as their definition of jealousy implies: " Romantic jealousy is a complex of thoughts, emotions, and actions that follows loss of or threat to self-esteem and/or the existence or quality of the romantic relationship" (White & Mullen, 1989, p. 9). During secondary appraisal, evaluation of the self relative to the rival or potential rival, plays an important role in the jealousy experience. As with Mathes, a variety of variables are identified as influencing the nature of the jealous experience in terms of the way the situation is appraised by the individual.

> If the partners motives are strong, if the rival has greater resources than the jealous person to satisfy those motives, if the jealous person is greatly dependent on the primary relationship, and if the potential loss is great, then the threat will be considerable. (White & Mullen, 1989, p. 43).

The way a situation is appraised and the choice of coping strategies that are ultimately employed will also depend, as Parrott (1991) suggests, on whether there is merely suspicion of an extra-dyadic relationship on the part of one's partner, or whether it has actually occurred.

The cognitive appraisal approach to jealousy provides a detailed and useful treatment of the proximate psychological mechanisms that underpin the experience of jealousy. As such, it provides an explanation of how the various kinds of variables associated with jealousy influence the way a situation is appraised and the kind of coping strategies that are employed. Factors such as relationship satisfaction and dependency are viewed as moderating variables on the kinds of secondary appraisals that an individual engages in. Cultural factors can also be nicely incorporated into the model as factors that influence primary appraisal and make salient different courses of action. The incorporation of self-esteem in the appraisal process by White and Mullen (1989) also provides a means of explaining the relevance of the properties of the rival to the way the situation is appraised and the kind of coping strategies that are implemented. Gender differences in jealousy are explained in terms of differences in sex roles and power in relationships. Because women have greater interpersonal skills they are more likely to focus on maintaining the relationship rather than adopting alternative strategies. Furthermore, because sex is more important to a man's self concept, sexual indiscretion on the part of the man's partner is more salient and becomes a more potent elicitor of jealousy (White & Mullen, 1989). The gender differences that are found between men and women, White and Mullen (1989, p. 132) claim are due to the differences in the degree and type of investments and rewards that each sex offers. "Females may be more likely to invest emotions and value intimacy, whereas males may be more likely to invest money and value sexual relations. Hence women may be more likely than men to link emotional investment and intimacy, and men may be more likely to link material input and sexual availability."

What the cognitive-appraisal approach does not really adequately explain is just why certain events, but not others, are appraised in a manner which leads to the experience of jealousy. That is, they do not provide us with an understanding of why the threat posed by a *rival* to one's relationship is universally appraised as a threat which leads to the *specific* experience of jealousy. The explanation of gender differences that are offered is also only partial in nature. It may be true that there are differences in sex roles and the kinds of things that women value, but this just represents a re-description of the phenomena that need to be explained. In other words, the cognitive approach fails to adequately explain why just *these* differences occur and why they occur in the context of a threat to one's primary relationship by an extra-dyadic affair.

In summary, cognitive approaches to jealousy provide a useful treatment of the proximate psychological processes which underlie the appraisal of a situation as jealousy-invoking and which lead to the range of cognitive and behavioral coping strategies. The variety of different variables which have been shown to modify the experience of jealousy can be incorporated into the cognitive model as factors which influence the appraisals undertaken and the choice of the appropriate coping strategy to implement. However, I would suggest that cognitive approaches only provide a partial explanation of the sorts of things that we need to account for in furthering our understanding of jealousy.

Social-cognitive approaches

A range of theories has also been developed from what might be loosely characterized as a social-cognitive perspective. Buunk (1991) for example, offers an exchange-theoretical perspective on jealousy. Following Kelley's (1986) interdependence theory of close relationships, Buunk suggests that people form close relationships in light of the rewards that such relationships offer. Partners are important to one another because they help each other obtain valued outcomes. Jealousy is experienced when the flow of rewards, especially exclusive rewards, that such relationships offer is threatened or disrupted by a rival.

Buunk's account focuses on the way norms and values influence and modify the nature of the jealousy experience. Partners abide by both societal and relationship norms, such as those regulating fairness and reciprocity. When these norms are violated, as in the context of an extra-dyadic relationship, the emotion of jealousy is experienced. The nature of the prevalent norms and the nature of the relationship itself will determine the strength and the nature of the jealous experience.

The exchange-theoretic perspective on jealousy is clearly compatible with both the evolutionary and cognitive approaches outlined earlier. People obtain things of value from a relationship and when a rival threatens that relationship the flow of those valuable resources is jeopardized. As such, the exchange-theoretic perspective details some of the proximate reasons for the experience of jealousy. A focus on the norms that are related to relationships also provides a means of explaining the differences between individuals in their experience of jealousy depending on the specific details of their relationship and the nature of prevalent norms in their culture. Gender differences in jealousy would be explained from this perspective, in terms of the different norms that govern male and female sexual behavior. The exchange-theoretic perspective, however, fails to adequately explain just why it is in the context of an extra-dyadic relationship on the part of one's partner that *jealousy* is experienced. Relationship break-ups for other reasons do not lead to jealous responses on the part of the individuals concerned even though the threat to the flow of resources is just the same if not greater. It is unclear why the specific violation of some norms rather than others lead to jealousy experiences. The exchange-theoretic perspective therefore fails to account for the *context-specificity* of jealousy. There also remains the question of just why certain norms are prevalent in relationships and in society in general. Why for example is the norm of exclusivity such a prominent feature of relationships? And why are there robust similarities in the kinds of gender-based norms that are seen in different cultures? The high incidence of jealousy among swinging couples is also difficult to explain from this perspective. Even in the presence of strict and acceptable norms regarding the *non*-exclusivity of extra-pair relationships, jealousy is still a common occurrence.

Salovey and Rothman (1991) offer another social-cognitive perspective on jealousy in their self-evaluation maintenance view of jealousy and envy. Basically it is proposed that jealousy is experienced, or most likely to be experienced, when an individual's relationship is threatened by a potential rival whose abilities in domains which are important to the individual are superior to the individual concerned. The instigation of jealousy, on this view, is primarily determined by a comparison between self and rival on whatever dimensions the individual views as a particularly salient in terms of their self-concept.

Based on Tesser's self-evaluation maintenance theory, this approach to jealousy recognizes the importance of comparison with the rival as a relevant modifying factor in the experience of jealousy. There is some attempt here, therefore, to provide an explanation of the context-specificity of jealousy. This domain-relevance approach can also provide some explanation of gender and cultural differences in the experience of jealousy as representing differences in the importance of domains which vary across cultures and which vary between men and women. Like the other theories of jealousy, however, there is no explanation of why there are stable and robust gender differences in just what domains are important for one's self-concept.

The self-evaluation maintenance view of jealousy also fails to explain the experience in jealousy by both men and women, when the rival or potential rival is *not* superior in any qualities to the individual concerned. Moreover, it is difficult to see why jealousy is experience by both sexes even when they have no idea who the rival actually is. The self-evaluation maintenance view also fails to adequately explain the range of tactics deployed by jealous individuals. Although the derogation of one's rival or the pursuit of alternative relationships makes sense from this perspective, it is difficult to see how aggression, mateguarding, desertion, and so forth operate to increase one's self-esteem. In summary, the self-evaluation maintenance view of jealousy, to my mind, really just serves to highlight one important aspect of the jealousy process rather than offering and broad and coherent explanatory account of the range of features associated with jealousy.

Socio-cultural approaches

Jealousy has also been approached from perspectives which focus on jealousy as a socio-cultural construction (Hupka, 1991). Hupka argues that jealousy as a process can be explained in terms of social structure and the social construction of the gender system. "The motive [for jealousy] is a product of the culture and can vary across cultures because it is an inescapable consequence of social structure" (Hupka, 1991 p. 261). Hupka suggests that individuals learn what is of most value in male-female relationships and therefore acquire the motive for jealousy. The experience of jealousy, therefore, is motivated when an individual experiences threats to learned values and customs. What is threatened in these cases is whatever is gained by being married to a partner in a given cultural context. "The cuckolded individual is responding to the betrayal of a norm of expectation regarding sexual behavior. In all societies the betrayal represents the loss of something of value as a result of the interlopers interferences" (Hupka, 1991, p. 265).

Hupka's approach, with its focus on social norms, is similar to many respects to the one offered by Buunk (1991) and can be evaluated in a similar way. A focus on specific features of different cultures is an essential part of many explanations of specific instances of jealousy. It is difficult, for example, to explain the severity of responses to female infidelity and potential infidelity in some cultures without discussing those features of the culture which makes these responses likely. The universal, or near universal, nature of the norms which regulate extra-dyadic relationships of course is *itself* in need of explanation. It is unclear just why this area of human relationships is regulated in similar manners across cultures. The approach of Hupka as with Buunk also fails to adequately explain the incidence of jealousy when under rare circumstances, these norms are relaxed. Although it may well be the case that jealousy is in some sense 'learned' we still need an explanation of why it is learned and why it is such a reliable feature of adult psychology.

Explaining jealousy: Relating the alternative perspectives

What is the best explanation for jealousy?

In the preceding pages I have outlined five different approaches to explaining jealousy. These different perspectives represent some of the main, but by no means only, theoretical attempts to understand the nature of jealousy. To the extent that these different approaches offer alternative ways of explaining jealousy we should be able to compare them in terms of their overall explanatory coherence (Thagard, 1992). Ideally it would be preferable to actually run the program ECHO.2 designed and implemented by Thagard to establish which approach is the most explanatorily coherent one. However, a pen and paper approach will provide some kind of indication of the general explanatory coherence of the different approaches.

Figures six and seven provide respectively an overview of the evidence that needs to be explained regarding jealousy and the main theories which have been developed to explain the evidence. Figure eight indicates which pieces of evidence that I believe each of the theories adequately explains. No doubt there would be much dispute over just whether or whether not each item *is* adequately explained by the various approaches. Herein in lies one of the main problems in comparative theory analysis. In many cases a theory or theoretical perspective will only partly explain a given phenomenon. I have somewhat crudely indicated this in my analysis by providing half marks for kinds of evidence only partially explained by the theories.

Figure six Jealousy-related evidence

E1 Jealousy is experienced only in the context of an extra-dyadic threat to a close relationship.

E2 Jealousy is an extremely powerful and aversive emotional experience.

E3 Jealousy is a cross-cultural universal experienced by both men and women.

E4 The experience of jealousy is associated with a variety of strategies, including:

- Mateguarding and vigilance
- Violence, aggression and homicide
- Improving quality of self and relationship
- Psychological derogation of partner and rival
- Desertion

E5 The experience of jealousy is influenced by a number of variables, including:

- Self-esteem and perceived adequacy as a partner
- Relationship dependency
- Attributes of the rival
- Quality of the relationship
- Relationship norms

E6 There are gender differences in jealousy, including:

- Men experience greater jealousy to situations involving sexual infidelity.
- Women experience greater jealousy in situations involving emotional infidelity.
- Men are more generally concerned about chastity and sexual unfaithfulness than women.
- Men are more likely to engage in strategies such as violence, vigilance and desertion.
- Women are more likely to engaged in strategies which emphasize emotional commitment and improving the quality of the primary relationship.

E7 Although universal the experience of jealousy and the range of different strategies employed by individuals varies across cultures. Examples of strategies related to jealousy employed in different cultures include infibulation, footbinding, claustration, veiling, and chaperoning.

E8 Jealousy is still experienced in the contexts of open marriages, swinging couples and in cultures which on some occasions condone extra-dyadic sexual relationships.

E9 Legal and social structures related to jealousy:
* Prevalence of laws that exonerate men for jealousy related homicide. Laws related to adultery typically focus on marital status of women.
* Prevalence of language which denotes marital and sexual status of women but not men.

Fig seven Overview of theories of jealousy.

Theories of Jealousy

Evolutionary approaches (T1)

Main hypotheses

- Species of organic beings have evolved and their characteristics are the product of natural selection.
- In species with paternal investment there will be strong selection pressures for mechanisms that are designed to assure paternity certainty and paternal investment. Jealousy in humans is just one such mechanism.
- Males and females should prefer mates who posses those attributes which further their reproductive goals in these respects.

Auxiliary hypothesis

- Some adaptations (such as jealousy) are faculative and their expression will vary in different contexts.

Cognitive-appraisal theories (T2)

Main Hypotheses

- Individuals are evaluative organisms who perceive the world in terms of their needs and desires.
- Emotion is the result of a series of appraisal and coping processes in which events in the world are evaluated in terms of their relationship to an individual's well-being.
- Jealousy is experienced when a situation is appraised as representing a threat to an individual's relationship
- Coping strategies are employed which reduce the threat to the relationship or to an individual's self-esteem.

Exchange-Theoretic perspective (T3)

Main hypotheses

- Individuals form and continue close relationships in light of the rewards that they offer
- Individual relationships are regulated by a series of norms and rules.
- Jealousy is experienced when a relationship is threatened and /or the rules and norms regulating the relationship are violated.

Auxiliary hypothesis

- Emotions in relationships are strongly influenced by specific features of the relationship such as attributional analysis and degree of dependency.

Self-evaluation maintenance theory (T4)

Main Hypotheses

- Individuals are motivated to maintain or increase self-esteem
- Self-esteem is strongly domain relevant. Threats to self-esteem are greater in areas of relevance to oneself
- Jealousy is experienced when a relationship is threatened by a rival whose characteristics in relevant domains are superior to one's own.

Socio-cultural approaches (T5)

Main Hypotheses

- The capacity for emotion is evolved
- All facets of emotion are the consequence of learning from one's social environment
- Jealousy is a social construction. It is the consequence of violations to socially learned values and norms regarding relationships.
- To the extent that different societies value different things and have different norms, jealousy should vary across cultures.

Figure Eight The comparative explanatory breadth of theories of jealousy

	Explanatory breadth score
T1	
Explains:	
E1, E2, E3, E4, E5, E6, E8	
Partially explains:	
E7, E9	**8**
T2	
Explains:	
E2, E5, E7, E8	
Partially explains:	
E3, E4, E6, E9	**6**
T3	
Explains:	
E2, E5, E7	
Partially explains:	
E3, E4, E6, E9	**5**
T4	
Explains:	
E8	
Partially explains:	
E1, E2, E3, E4, E5, E6, E7, E9	**5**
T5	
Explains:	
E7	
Partially explains:	
E1, E2, E3, E4, E5, E6, E9	**4.5**

It is clear from figure eight that the evolutionary programme offers the most unifying theoretical account of the various features of jealousy that we need to explain. Other considerations suggest that it also offers the most explanatorily coherent one as well. Firstly, the evolutionary programme explains a wider range of phenomena than do alternative approaches; that is, it has greater explanatory breadth (Thagard's principle 2.1). This is especially true once we consider the wide range of different strategies employed by a variety of different species to solve similar adaptive problems that the evolutionary programme also explains. By drawing on the conceptual resources of parental investment theory, sexual selection theory, and sperm-competition theory, the evolutionary programme can explain not only those psychological and physiological adaptations that humans possess, but also the range of traits possessed by other species which function in similar contexts. By contrast, the socio-cultural approach offered by Hupka or the exchange-theoretic approach of Buunk, by emphasising the role of social norms and values, cannot adequately account for the presence of these mechanisms across different species. It should be noted here, however, that many of the other approaches, for example the cognitive appraisal approach, draw on theories which are more generally applied to emotion and hence do have reasonably broad scope. This is also true of the evolutionary research programme more generally in that it offers a general theoretical approach to emotion.

The evolutionary approach also gains coherence through the *explanation* of the theories of parental investment and sexual selection which are used in turn to explain jealousy (Thagard's principle 2.2). These theories are themselves explained by inclusive fitness theory and the hard core of the evolutionary programme. The coherence of the evolutionary research programme is further increased via Thagard's third principle of explanatory coherence, analogy. The idea that the mechanisms underling jealousy are evolutionary adaptations gains greater acceptance thorough demonstrating that analogous mechanism have evolved in other animals for similar reasons.

Incoherence between alternative explanatory schemes may also occur when evaluating the different approaches to jealousy. For example, the idea that gender differences in jealousy are purely the result of contemporary social structure and due to a general learning of gender-based norms is incoherent (in this format at least) with the suggestion that gender differences represent the consequence of different selection pressures on human males and females in the context of partner infidelity. I think it is clear that the alternative theories do compete with one another in some areas. However, as I will elaborate below, there is also a considerable degree of consistency between the different approaches. In fact the different approaches should be, to some extent, conceived of as being explanatorily *connected.*

My use of Thagard's theory of explanatory coherence is somewhat informal here. However, I think that it does indicate that an evolutionary approach to jealousy provides a coherent and unifying scheme for furthering our understanding of the nature of jealousy. By itself though, the evolutionary programme is insufficient for explaining all the relevant aspects of jealousy, especially the details of *specific* jealous experiences. To achieve this aim it necessary to consider how the alternative approaches to jealousy might be conceptually related to one another and to examine just what sort of explanations that they provide.

The relations between alternative approaches

DeSteno and Salovey (1994) in their summary of the different perspectives on jealousy argue that alternative theoretical approaches should be considered as supplementary rather than antagonistic and that all the various approaches help to increase our understanding of jealousy. I agree here with the general sentiment expressed by Desteno and Salovey (1994), although I believe that the different approaches are not entirely compatible with one another and that we should, while accepting the value of theoretical pluralism, be rather less egalitarian in our treatment of alternative perspectives.

In chapter one I discussed some of the different ways that the relationships between theories can be conceptualized. In comparing an evolutionary approach to alternative perspectives[5] I think it is clear that the theories are not entirely *compatible* with one

[5] This comparison is best done on a pair-wise basis, however, I generalise my evaluation between evolutionary approaches and the rest of the alternative theories. In general the conceptual relation between evolutionary approaches and any of the other theories will be roughly the same.

another, because evolutionary theory does *entail* something about the psychological and psycho-social processes operating in jealousy, which is the focus of the alternative approaches. For example, evolutionary theory suggests what factors are likely to be psychologically salient and indicates what sort of events are likely to be appraised as harmful or threatening. Evolutionary theory can also not be said to completely absorb or *incorporate* alternative theories because at some points, as indicated above, they are incompatible with one another. Furthermore, evolutionary approaches only furnish us with partial explanations of some phenomena.

I would suggest that the best way of understanding the relationship between evolutionary approaches and alternative theories is to consider the relationship to be one of *sublation* or *reinforcement*. The evolutionary research programme partially incorporates and provides a rationale for the alternative theories while rejecting some of their assumptions. For example, an evolutionary approach can explain why the three constructs of commitment, insecurity, and arousability proposed by Bringle's (1991) transactional model are important in the context of jealousy-invoking situations while rejecting the idea presented by Bringle that there are no *a priori* situations that are jealousy invoking and that jealousy in entirely a social construction.

A clearer understanding of the relationships between the different theories of jealousy can also be profitably achieved by examining the levels on which the explanations operate and the kinds of explanation that they afford us.

In chapter two I discussed the various levels at which explanations in psychology can be conceptualized. These levels include the functional, the physiological, the psychological and the social. In developing complete explanations of jealousy, ideally we should consider the function of jealousy (in evolutionary terms), the physiological basis of jealousy, the various psychological processes underlying jealousy and the relations between these processes and the social environment. One way of relating the alternative theories of jealousy, therefore, would be to view them as primarily addressing different levels of analysis. The evolutionary approach helps to explain the function of jealousy, the cognitive approaches delineate the relevant psychological processes, while the socio-cognitive and socio-cultural approaches help us to understand the relations between individual psychological processes and the social environment.

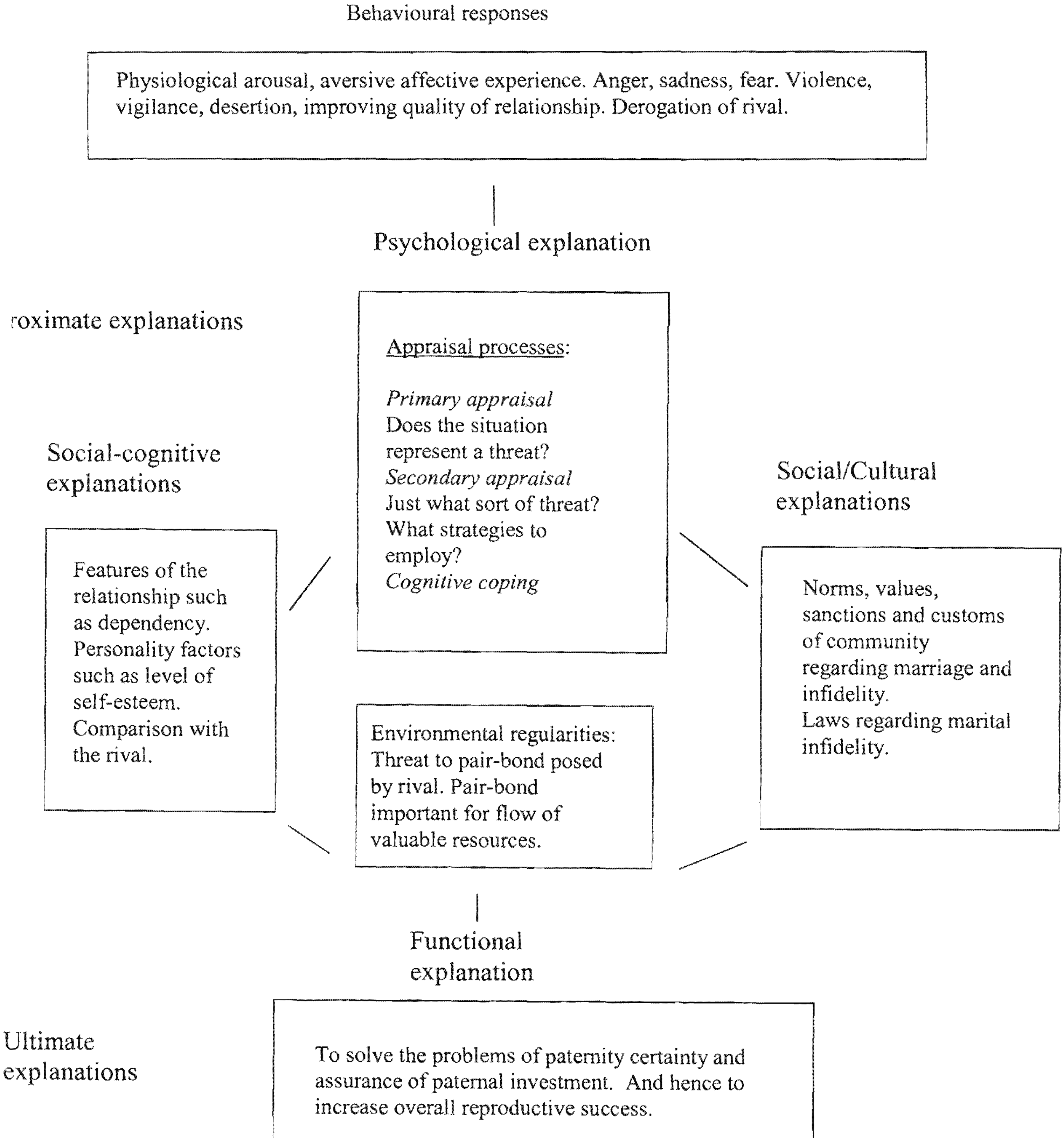
Furthermore, we might view the evolutionary approach as offering us the *ultimate* explanation for jealousy, whereas the various other theories provide details of the more *proximal* mechanisms that mediate jealousy responses. Consider for example the following series of hierarchically nested why questions and answers:

Q: Why did Bob kill his wife's lover?
A: Because Bob was jealous.
Q: Why was Bob jealous?
A: Because Bob found his wife's lover in bed with his wife.
Q: Why did this make Bob jealous
A: Because the presence of a sexual rival threatened Bob's relationship with his wife.
Q: Why does this lead to jealousy?
A: Because those individuals ancestral to Bob who experienced jealousy in such situations and responded in certain ways were more likely to be reproductively successful.

In this series of questions we move from proximate explanations in terms of antecedent conditions and the cognitive appraisals of Bob, to ultimate explanations of the causal processes responsible for these patterns of appraisal to certain conditions. Just which explanation we may prefer at any given time will depend in part on pragmatic issues. Moreover, we may want to know why Bob *murdered* his wife's lover rather than choosing some alternative course of action. This explanation may draw heavily on some of the important proximate causes operating in the specific situation such as the details of Bob's personality and the salience of the event in terms of Bob's relationship with his wife.

For a complete explanation of jealousy we need to consider the full range of causal processes operating at different levels of analysis and over different chronological frames. Moreover, we need to consider how these different explanations might be integrated with one another to provide a full causal picture. Figure nine outlines the explanatory relations operating between different levels of analysis in attempts to

Figure nine Model of romantic jealousy detailing levels of analysis and explanatory relations



explain jealousy. Ultimate explanations for jealousy drawn from the evolutionary research programme help to explain some of the proximate causes of jealousy such as those embodied in appraisal processes. Evolutionary explanations, however, will also have to draw on specific features of the intra-personal, social and cultural contexts in order to explain the specific patterns of thought and behaviour that are elicited in any given situation. For example, the acceptability in some cultures of homicide as a reasonable response to sexual infidelity coupled with high levels of arousability in an individual will help us to explain why murder was appraised by that person to be an acceptable method for responding to a situation involving an extra-dyadic relationship on the part of their partner.

In a general overview of cultural variations in emotion Mesquita and Frijda (1992) suggest that different aspects of the emotional experience are likely to differ to varying extents across cultures. For example, cross-cultural similarities are likely to occur in terms of universal response modes and universal event types. Differences between cultures are likely to be demonstrated in terms of specific forms of behavior and different appraisal propensities. For jealousy there are cross-cultural universals in terms of the context of jealous experiences and the aversiveness of these experiences. Cultural variation is reflected in terms of different appraisal processes and different patterns of response. The norms, rules, and laws regulating behavior in different cultures is likely to make salient different courses of action in the context of sexual and emotional infidelity.

Once we start to explain specific phenomenon which are culturally localised, our explanation will draw heavily on the specific features of the culture and the belief structure of the community. The practice of infibulation, for example, is often mentioned in the context of evolutionary explanations of jealousy and explained as a consequence of evolved male psychologies (e.g., Daly et al., 1982; Buss, 1994). However, although the evolved psychological mechanisms that males possess must play some role in a satisfactory explanation of infibulation, we will also need to draw heavily on the beliefs of both men and women in infibulating cultures and the source of those beliefs in terms of specific features of the culture, such as the widespread influence of Islam.

In general, I think the most fruitful way of conceptualizing the role of evolutionary approaches to jealousy is to see the evolutionary research programme as providing a *unifying explanatory framework* for the general phenomena related to jealousy. By instantiating the few argument patterns associated with adaptation explanations and by drawing on the specific auxiliary theories of the evolutionary programme we can unify a wide range of different kinds of phenomena in both humans and other animals related to the problems of paternity certainty and paternal investment. The evolutionary research programme provides a means of understanding why jealousy exists, why it occurs in certain narrowly delineated situations, and why there are important gender differences in the kinds of situations that elicit jealousy responses. We also, however, need specific *causal-mechanistic explanations* of the various processes that bring about the experience of jealousy. The cognitive appraisal approach is one such attempt at providing the details of the psychological mechanisms which underlie jealousy. Similarly, a consideration of psycho-social mechanisms can also further our understanding of jealousy by showing how jealousy is influenced by various processes of a social nature.

It should be clear from my discussion in chapter one that these two approaches to explanation – explanation as unification and explanation as elucidation of causal mechanisms - are essentially connected with one another. Unifying explanations tell us why certain patterns and processes exist in the world by drawing on theories with broad scope, while causal-mechanistic explanations detail just how these processes are instantiated in any given instance. To explain jealousy in *general* we may want to take a specifically evolutionary approach, while our explanations of *particular* instances of jealousy may draw more heavily on the various theories which delineate the causal mechanisms which underlie jealousy experiences.

Because humans exhibit a high degree of plasticity, we would expect a diversity of different responses to jealousy invoking situations. We need to pay due attention, therefore, to the specific features of the individuals concerned and the relevant local details of their environment. It is towards an attempt at explaining these variables that most theories of jealousy developed by psychologists have been directed. Indeed, although I may not have done full justice to the approaches that I have discussed, it seems to me that perspectives like the transactional model and the socio-cultural approach really represent a summary of some of the important variables associated with jealousy rather than being well-articulated theories. That is, they appear to be predominantly descriptions of empirical regularities rather than truly explanatory theories. This by itself is not a bad thing. Indeed, the elucidation of phenomena is an important part of the scientific process. However, it is important that we go beyond such local descriptive work to develop integrated accounts of the causal mechanisms which underpin jealousy as well as detailing the ultimate explanations for why particular mechanisms which serve specific functions are instantiated in humans.

Pragmatic concerns

Just what sort of explanation that one seeks regarding jealousy will depend, to some extent, on the specific question that is being asked. That is, what is accepted as an adequate explanation is partly a question of *salience*. For example, consider how the following questions direct attention to specific features of the jealousy system and hence require different kinds of explanation.

(1) Why do *humans* get *jealous* when they discover *sexual* infidelity of the part of their *partner*?

This question, I would suggest, requires an explanation drawn specifically from the evolutionary research programme. In answering the question above we need to be able to provide an answer which tells us just why jealousy is experienced in this context but not others. By referring to the evolutionary history of humans and the specific adaptations that they possess we can furnish an explanation which satisfies these specific epistemic needs.

(2) Why did *Bob* not want his *wife* to meet his attractive and intelligent friend Hamish?

One reasonable answer to this question would be to suggest that Bob experiences low self-esteem, perceives himself to be inadequate in certain ways relative to Hamish, and therefore feels threatened by the possibility of Hamish meeting his wife. In order to answer this question we need to draw on the specific details of Bob's personality and the kinds of situations that often elicit jealous responses.

(3) Why did Javed pour boiling water over his wife's face when he discovered her having a sexual relationship with another man?

Our explanation here is likely to draw on specific features of Javed's culture, such as the lack of sanctions against spousal violence in the context of extra-dyadic relationships. We may also want to invoke Javed's high level of trait jealousy and arousability and his specific beliefs regarding women and their role in the marriage. In other words we answer this question in terms of the specific details of the social environment and the psychological development of the individual in this context.

In answering the last two questions we simply assume that jealousy exists and that it occurs in some situations and not others and that it can lead to certain courses of action such as mate vigilance or violence. We do not specifically ask why this happens to be the case.

These three questions can all be viewed as requests for *explanatory information*; information which represents part of the ideal explanatory text. A *complete* answer to any of the three questions, would require the full explanatory details offered by the evolutionary research programme in conjunction with the relevant specific theories of the causal mechanisms as well as the details regarding important contextual features.

In our treatment of some of the moral issues relating to jealousy we need to pay due attention to full causal-explanatory story even though it may appear more relevant in some contexts to focus only on portions of the ideal explanatory text. If we want to take a reasonable clinical approach to ameliorating some of the specific deleterious consequences of jealousy it is helpful to have an idea of the larger explanatory picture. A useful clinical strategy, given what we know about jealousy, would not be to try and eliminate it. Understanding the function of jealousy from an evolutionary perspective provides a way of conceptualising jealous experiences as a normal and perhaps inevitable part of romantic relationships. What is *not* inevitable is the specific course of action taken by individuals in any given situation. It is here that a detailed understanding of the underlying cognitive and social mechanisms provides a way of allowing us to intervene in the relevant mechanisms so as to bring about desirable results. For example, if self-esteem and relationship dependency are relatively important variables which determine the extremity of the jealous response, therapeutic efforts might be helpfully directed at increasing autonomy or bolstering self-esteem. Similarly, changes to legal sanctions and cultural norms, while not eliminating jealousy, in some societies may reduce the amount and severity of jealousy induced violent behavior.

Summary

The phenomenon of jealousy, like many in psychology, has led to the development of a plethora of theories which attempt to increase our understanding of why jealousy occurs and why it has the specific features that it does. My suggestion here is that in developing a complete explanatory account of what jealousy is we must develop appropriately integrated and coherent theoretical perspectives. The evolutionary research programme plays a central role in furthering our understanding of jealousy by providing an understanding of why jealousy exists and why certain mechanisms have developed which underlie jealous experiences in certain specific environmental contexts. The various other theoretical approaches offer specific details of the causal mechanisms underlying jealousy and provide descriptions of the key variables which mediate the experience of jealousy.

The evolutionary research programme can be viewed as sublating these various approaches in that it provides a rationale for the specific theories and relevant variables, while rejecting some of their assumptions. The general explanatory strategy here, I suggest, is that we should deploy the evolutionary research programme in conjunction with a range of specific and local theories in our attempts to further our understanding of this and other emotions. The range of theories developed by psychologists to explain jealousy are not necessarily absorbed by the evolutionary programme but are instead, combined with, and partly explained by, the general evolutionary research programme.

Chapter ten

The role of evolutionary explanations in psychology

The current status of psychological science

Over the course of the twentieth century we have witnessed considerable progress in many areas of science. The implications for technology, society, and the environment have been equally considerable, if not always beneficial. For example, since the discovery of the structure of DNA by Watson and Crick in 1953, there have been dramatic advances in the field of genetics. In less than fifty years since Watson and Crick's discovery, the sequencing of the human genome has not only become a theoretical possibility, but is also near to becoming an accomplished fact. The implications of this research for humanity in general are profound and far-reaching.

By contrast, progress in psychology and the other social sciences has been slow, and their impact on society, although not negligible, has been comparatively less than that of the physical and biological sciences. As I discussed in chapter two, the lack of progress demonstrated by psychology can be attributed to a range of methodological, conceptual and institutional problems. Psychologists, as Meehl (1967, 1978) has repeatedly made clear, use inappropriate and inadequate methods in their investigation of their subject matter. These methodological woes are abetted by institutional features which promote quantity over quality of research (Lykken, 1991). Conceptually, psychology remains disunified and is characterised by a proliferation of small-scale theories of limited scope and applicability (Royce, 1985; Staats, 1989).

Science in general can be reasonably characterised as a cumulative affair. Over time we develop better and better models of the world; models that further our understanding of nature and of our place in it. In psychology, however, there has been little conceptual progress. This point is made clear by Lykken (1991, p. 7):

> In the hard sciences, each generation stands upon the shoulders of its predecessors, the bones of the Elder Giants become part of the foundation of an ever-growing edifice. The great names of psychology's comparatively recent past are respected mainly as intrepid explorers who came back empty-handed. There is no edifice, just this year's anthill, most of which will be abandoned and washed away in another season.

"The present state of knowledge in psychology" Lykken (1991, p. 7) concludes is "very broad but shallow. We know a little bit about a lot of things."

Evolutionary psychologists in their critiques of contemporary psychology come to a similar conclusion to that reached by Lykken and others (e.g., Staats, 1989). For example, Tooby and Cosmides (1992, p. 23) suggest that:

> After more than a century, the social sciences are still adrift, with an enormous mass of half-digested observations, a not inconsiderable body of empirical generalizations, and a contradictory stew of ungrounded, middle-level theories expressed in a Babel of incommensurate technical lexicons.

Evolutionary psychologists argue that progress in psychology has been slow because of a failure to realise that psychology is really just a branch of biology (Tooby & Cosmides, 1989). Evolutionary psychology, it is suggested, offers just the kind of conceptual integration that psychology requires to emerge from its chaotic and non-progressive state. As Buss (1995, p.20) states: "Evolutionary psychology provides the conceptual tools for emerging from the fragmented state of current psychological science." The key point suggested here is that the evolutionary programme offers to *unify* psychological science under the umbrella of single paradigm or metatheory.

This point has been made by many who have offered evolutionary explanations of psychological and social phenomena. Wilson (1975), for example, clearly presented sociobiology as a unifying scheme for the social sciences. Similarly, Barash (1982) and Van de Berghe (1983) claim that sociobiology offers a unifying framework which represents a new paradigm for psychology in the Kuhnian sense. Buss (1995, p. 85) reaches a similar conclusion in his claim that "evolutionary psychology is a revolutionary new paradigm". The strong implication here is that sociobiology and evolutionary psychology offer a way of overthrowing or replacing contemporary psychological theorising with explanations drawn explicitly from the evolutionary research programme.

> Although evolutionary psychology is in its embryonic stages, we hope and expect that its growth will eventually replace the welter of conflicting middle-range psychological theories and the wealth of descriptive information with a series of models of the innate mechanisms that comprise the human psyche. (Tooby & Cosmides, 1989, p. 33).

What are we to make of these bold claims for conceptual revolution?[1] Will a future psychology be practiced as a branch of biology with a strong commitment to develop evolutionary explanations of psychological and social phenomena? And will the current proliferation of theory in psychology be replaced by theories drawn explicitly from the evolutionary research programme?

In the remainder of this chapter I will discuss the various claims that the evolutionary programme offers a revolutionary conceptual paradigm which could serve to replace existing theoretical schemes in psychology. I shall suggest that there are various reasons why we should reject this strong claim, while maintaining that evolutionary explanations will have an important role to play in the future of psychology. The precise nature of this role will be outlined and the ways that the evolutionary research programme might further progress in psychology will be suggested. My conclusions in this chapter will draw on the conceptual resources related to theory appraisal, research programmes, explanation, and scientific progress outlined in the first two chapters.

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[1] Just how evolutionary explanations should figure in psychology and the relationship between evolutionary theory and other theories in psychology seems to vary considerably from author to author and even in the same work by the same author. The claim for conceptual revolution represents the extreme position here and is probably not the one held by the majority of those who claim allegiance to the evolutionary research programme in psychology.

The explanatory coherence of the evolutionary research programme in psychology: past, present, and future

Firstly, and most importantly, it is worth emphasising that neither sociobiology nor evolutionary psychology offers a *new* paradigm as such. As Ruse (1987) has made clear, sociobiology is not a new paradigm, but is merely an expanded branch of the Neo-Darwinian theory of evolution. The same is clearly true for evolutionary psychology. As I discussed in chapter three, both sociobiology and evolutionary psychology draw on the conceptual resources of the general evolutionary research programme. It is certainly true that the proliferation of important theories developed by Hamilton, Trivers and Maynard-Smith in the 1960's and 1970's afforded a considerable degree of conceptual advancement, especially in terms of explaining social behaviour. However, these theories are best considered as auxiliary theories that augment the hard core of the general evolutionary research programme, rather than offering a new paradigm as such.

It is also worth pointing out that attempts to explain human psychological and social phenomena are not by any means new. Ever since Darwin formulated his principle of natural selection, there have been diverse and widespread attempts to use evolutionary explanations to further our understanding or psychological and social processes. Any claims that sociobiology or evolutionary psychology offers a new paradigm for psychology, or that psychology is best conceived as a branch of biology, therefore, must demonstrate why this should be the case now, when such attempts are more than a hundred years old. That is, it needs to be shown that the evolutionary programme is the most explanatorily coherent approach to psychology and that judgements of explanatory coherence have changed, or need to change, since earlier attempts to develop evolutionary approaches to psychology.

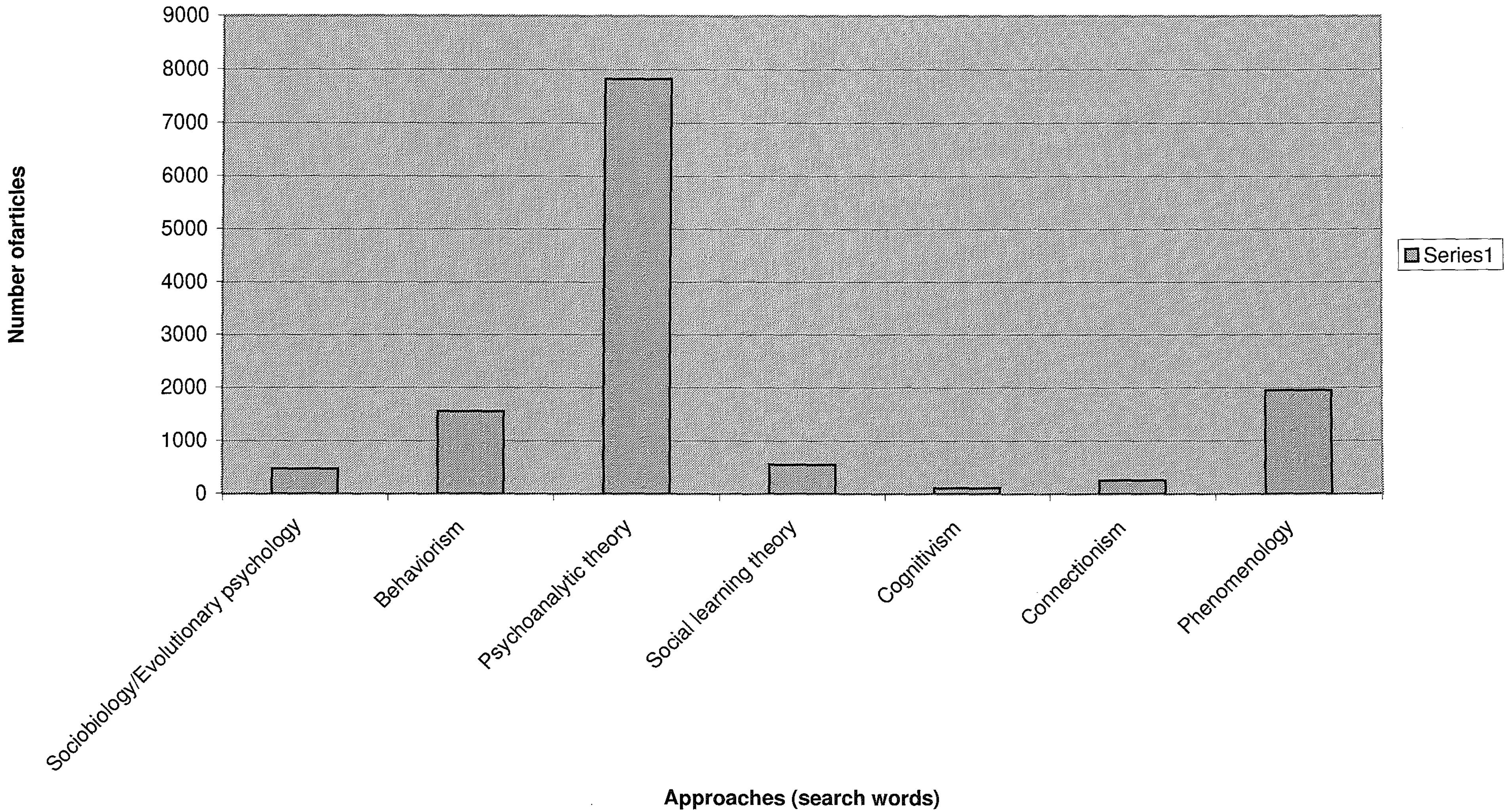
It is clear that evolutionary approaches in psychology do not represent the mainstream view held by psychologists. That is, contemporary psychologists, in general, do not perceive evolutionary theory to be the best way of explaining psychological and social phenomena. This point is demonstrated in Figures ten and eleven. Using *Psychlit*, an on line reference base, searches under a range of theorists and theoretical approaches demonstrate that references to Darwin and Evolutionary theory are substantially less than those to behaviorism or psychoanalytic theory or to Freud and Piaget. Clearly psychologists are more likely to draw on theorists and theoretical approaches in their written publications other than Darwin and evolutionary theory. Moreover, as figure twelve illustrates, there has be no substantial increase in references to either sociobiology or evolutionary psychology since the early 1980's.

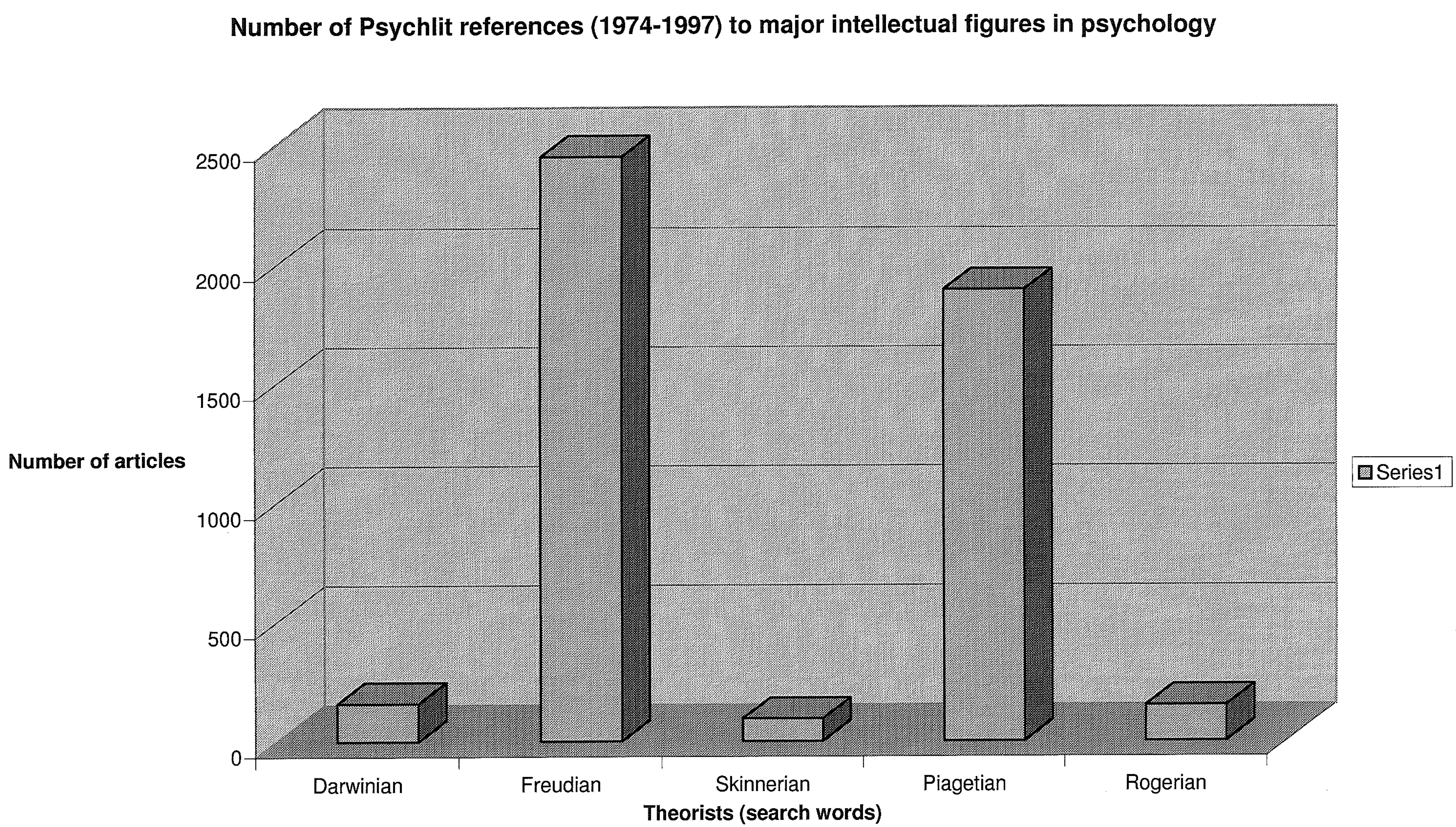
No doubt these *psychlit* searches offer a rather crude measure of interest in various theoretical approaches in psychology and cannot tell us what the *best* approach is. Certainly there is some indication that evolutionary approaches in psychology are becoming more prominent, especially in some domains such as social psychology (e.g., Simpson & Kenrick, 1996). Moreover, evolutionary approaches have expanded to encompass a huge range of different areas in psychology from psychiatry (Stevens & Price, 1996) to neuroscience (Gazzaniga, 1992). However, it is clear that if the evolutionary programme does offer a revolutionary new approach, it is a revolution which shows little sign of widespread acceptance in the near future.

In chapter three, I presented a brief overview of the history of the evolutionary research programme in psychology. I argued in this chapter that we could understand the rise and decline in the prevalence of evolutionary explanations in psychology in terms of the perceived acceptability of the evolutionary programme in which questions of explanatory coherence are prominent. The original interest in evolutionary explanations in psychology can be understood in terms of the promise of explanatory unification. Darwin's theory of evolution made clear that humans and other animals, mind and behaviour, are all subject to the forces of evolution embodied in the process of natural selection.

The subsequent decline of evolutionary explanations in psychology can also be traced to a variety of sources. Questions regarding the moral acceptability of the programme, the programme's explanatory scope, and questions over the use of analogical reasoning all contributed to the evolutionary programme in psychology being perceived as less acceptable. The rise of behaviorism as an alternative theoretical

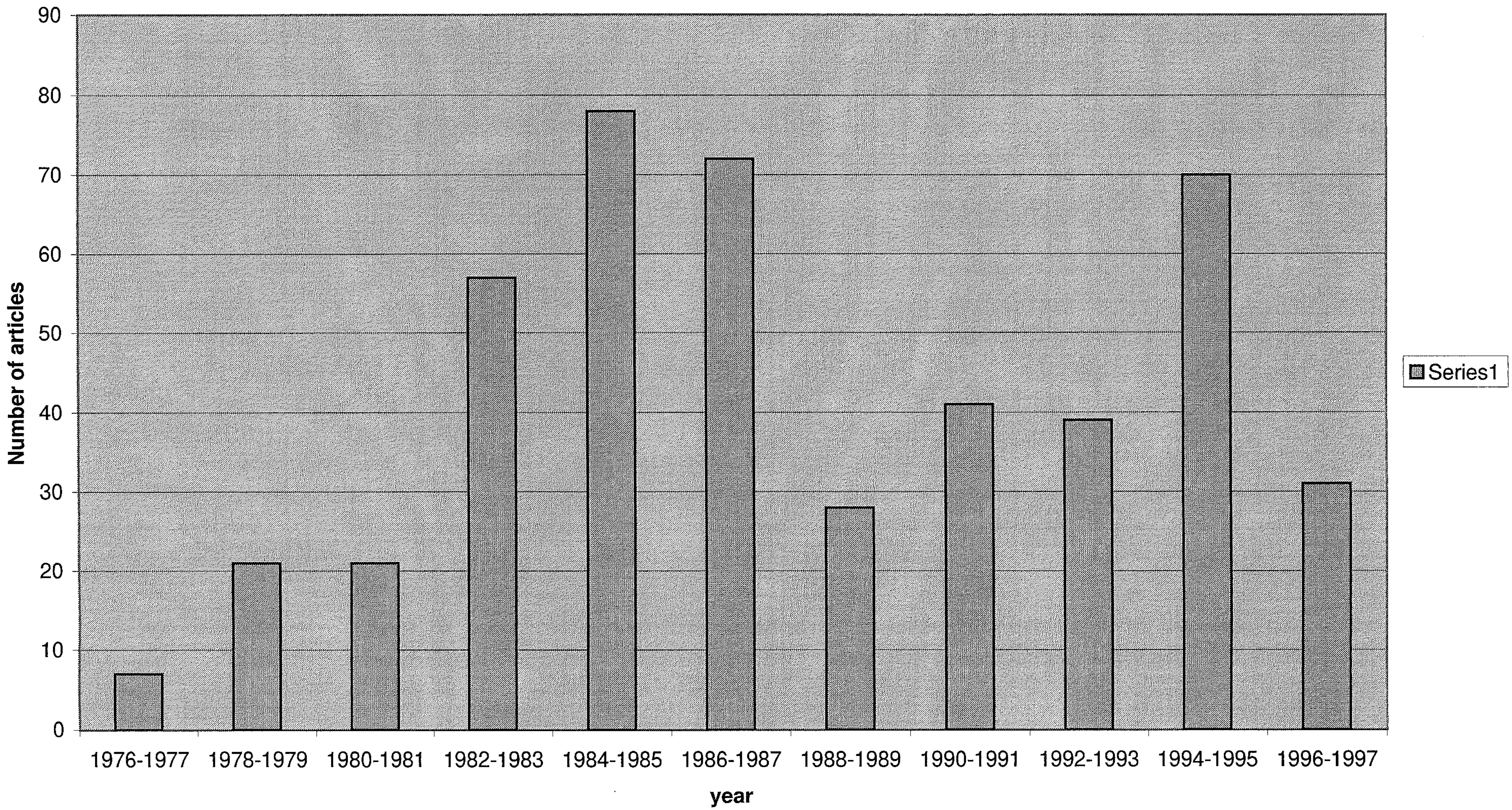
Number of psyclit references (1974-1997) to major approaches in psychology





Number of Psychlit references (1974-1997) to major intellectual figures in psychology
Number of articles
2500
2000
1500
1000
500
0
Series1
Darwinian
Freudian
Skinnerian
Piagetian
Rogerian
Theorists (search words)

Number of journal articles refering to sociobiology and evolutionary psychology from 1976-1997.



approach also played an important role in the decline of evolutionary explanations in psychology.

The return of evolutionary explanations in psychology can also be traced to a variety of factors, which overall can be conceptualised as increasing the explanatory coherence of the programme relative to competing research programmes.

Firstly, since the 1950's, a revival of instinct as a potential explanatory source of behaviour increased the scope of the evolutionary programme relative to behaviorism. If a large number of behaviours could be best explained as species specific adaptations rather than reflecting a generalised learning history, then evolutionary theory could potentially explain a wider range of phenomena than previously thought. Purely operant and respondent learning explanations of behaviour, therefore, became less viable accounts of the range of natural behaviours studied by ethologists, and comparative psychologists.

Since the mid 1950's there was also an increase in the number of studies of primate behaviour under natural conditions (Wilson, 1994). An increased understanding of primate behaviour and the evolutionary significance of animal behaviour more generally, strengthened by analogy, the possibility of evolutionary explanations of human behaviour. As more detailed accounts of primate and especially chimpanzee behaviour and cognition began to emerge, the similarities between humans and other animals became more apparent, especially in the social sphere. Like humans, chimpanzees were revealed to live in complex social groups, hunt for meat, engage in food sharing, and use tools (e.g. Goodall, 1986)

Another important factor in the re-emergence of evolutionary explanations in psychology and the social sciences was the development of a range of auxiliary theories in the 1970's by Trivers, Hamilton and Maynard-Smith, which helped to provide increasingly satisfactory evolutionary explanations of a broad range of phenomena, especially social phenomena. This growing interest in the role of evolutionary explanations in the social sciences was reflected in the launching of several journals such as *Ethology and Sociobiology, Behavioural Ecology and*

Sociobiology, and *Human Nature,* all devoted to articles developing evolutionary explanations of human and animal behaviour.

It is unlikely that the reemergence of evolutionary explanations in psychology can be attributed in any significant way, to social and political factors. As Degler (1991) points out, the resurgence of evolutionary accounts of human behaviour emerged in the 1960's and 1970's at a time when liberalism in politics was widespread. Indeed, the somewhat negative reception of sociobiology by liberal political groups and like-minded scientists bears testimony to the unfavorable political climate in which the new developments emerged. It would seem then that a variety of a factors linked to the explanatory coherence of the evolutionary programme can account for its re-emergence in psychology. In particular, the breadth of the programme was extended, or more specifically, increasingly satisfactory explanations were developed for a wider class of phenomena relative to competing explanatory accounts such as behaviourism. Empirical and conceptual progress in ethology has also contributed to the coherence of the programme by providing a richer source of analogy from which to draw upon. However, despite the increase in interest in evolutionary explanations in psychology since the 1970's, they have not as yet received widespread acceptance.

Advances in biology more generally at both an empirical and conceptual level suggest that the evolutionary research programme in psychology is more explanatorily coherent than ever it has been. The evolutionary programme certainly promises the most unifying explanatory scheme available to psychologists. Although questions of explanatory breadth are hard to determine on a global scale, the sheer range and diversity of phenomena in psychology and the social sciences which have been explained from an evolutionary perspective is indicative of the wide scope of the programme. Evolutionary explanations have been used to explain phenomena in perceptual psychology (e.g., Shepard, 1984, 1987, 1992), cognitive psychology (e.g., Cosmides & Tooby, 1989, Cosmides & Tooby, 1992; Baron-Cohen, 1995; Pinker, 1994) social psychology (e.g., Simpson & Kenrick, 1996) neuropsychology (e.g., Gazzaniga, 1992), developmental psychology (e.g., Hazen & Shaver, 1987; McDonald, 1992), emotion (Plutchik, 1980) and clinical psychology (Marks, 1987, Stevens & Price, 1996) among others. Certainly no other theoretical approaches in psychology display this kind of explanatory breadth. Moreover, the kinds of explanations that are deployed typically use few special assumptions and invoke the same kind of argument patterns which are used to explain biological phenomena in general.

More and more detailed studies of animal mind and behaviour have also increased the explanatory coherence of the evolutionary programme by providing richer sources of analogy to draw on in the development of increasingly more adequate adaptation explanations of human psychological phenomena. The burgeoning research on social cognition in group living primates, for example, is drawn upon in the development of evolutionary explanations of human social behaviour and in the discussion of specific adaptations designed for group living. The evolutionary research programme also displays a high degree of internal coherence or consistency in that specific theories in the programme are mutually consistent with other theories and ultimately with the principle of natural selection at the hard core of the programme.

Given the high degree of explanatory coherence manifested by the evolutionary research programme what reasons can be forwarded for its lack of widespread acceptance in psychology? One reason that has been, and to some extent still remains, prevalent is related to concerns over the moral implications of the evolutionary research programme in psychology. Evolutionary explanations have often been associated with right-wing political positions and a deterministic view of human nature. However, I see no good reason why this should necessarily be the case. In fact I think it is just as easy to use evolutionary theory to support left-wing politics and liberal causes. For example, one might stress the important role of sociality, cooperation and group cohesiveness in the evolution of social species to support the importance of harmonious community life, or perhaps emphasise the continuity between humans and other animals in a defense of animal rights, or point out the ecological interconnectiveness of all life in the development of an environmental ethic.

The more general problem in the context of perceive moral unacceptability is that psychologists and social scientists, typically speaking, have a poor general understanding of evolutionary theory and of the nature of evolutionary explanations. The evolutionary research programme, therefore, is perceived to be less explanatorily coherent than it otherwise might be. In order to evaluate the epistemic value of an evolutionary research programme, as many have noted (e.g., Kuhn, 1962, Thagard, 1992, Giere, 1988), it is essential to fully get 'inside' the programme and to clearly understand it's conceptual structure. The way psychologists are educated typically precludes the development of such an understanding of evolutionary theory. Although there are courses in evolutionary psychology, comparative psychology and related subjects, these are neither compulsory nor widespread.[2] It is no wonder that psychologists typically emerge with a poor understanding of the nature of evolutionary theory.[3]

The lack of understanding of evolutionary theory by psychologists clearly contributes to the perceived inadequacy of the evolutionary programme in psychology, but I would suggest it is not the only or even primary reason. My discussion of the various challenges to the evolutionary programme in psychology in chapters four to nine suggest that evolutionary explanations have an important but not exclusive role to play in furthering our understanding of psychological phenomena. A clearer understanding of the perceived inadequacy of the evolutionary programme can be obtained by a focus on the kinds of explanations that it offers us and its relations to other kinds of explanations in psychology. The lack of acceptability of the evolutionary research programme in psychology can be traced to, among other things, questions regarding the programme's external coherence. That is, there is considerable confusion among psychologists about the way that evolutionary explanations might be related to other sorts of explanations in psychology.

Levels of analysis and evolutionary explanations in psychology

In chapter two I outlined a frequently used distinction between the different levels of analysis which explanations in psychology can be drawn from. We can simultaneously provide functional, physiological, psychological, and social explanations for many phenomena of interest to psychologists. Moreover, the

[2] The same could be said for other important areas of psychology such as theoretical and historical psychology, with similar results.
[3] I do not mean this, in any sense, as a criticism of psychologists, but more as a general point regarding some of the institutional reasons for the lack of acceptability of evolutionary explanations in psychology.

different kinds of explanations that we employ can be conceptualised as being either ultimate or proximate in nature. Similarly, the ethologist Niko Tinbergen (1963) has provided an influential distinction between four types of why questions that we can direct at biological systems. We can legitimately ask questions of a functional, phylogenetic, ontogenetic, and mechanistic nature. In explaining some biological entity we can describe its adaptive function, its evolutionary history, its developmental trajectory and the proximate physiological and psychological mechanisms which produce it. These various distinctions, based on levels of spatial and temporal analysis, immediately suggest one way that we might understand the role of evolutionary explanations in psychology. Perhaps the evolutionary programme can be seen to furnish us with functional and phylogenetic explanations, that is ultimate ones, whereas mainstream psychological theory provides us with proximate explanations of the relevant developmental, psychological, social and physiological processes which underlie the phenomena of interest. Differences of opinion emerge, however, in terms of just how these different kinds of explanations are related to one another. Roughly speaking, there are four different stances that could be taken on the relevance and role of ultimate explanations in psychology. These different positions suggest different roles for the evolutionary programme in psychology and indicate different beliefs about the relation of evolutionary explanations to other sorts of explanations in psychology.

(1) Proximate explanations of human psychological phenomena screen off ultimate explanations. Evolutionary theory, therefore, is explanatorily irrelevant in furthering our understanding of psychological and social facts.
(2) Evolutionary explanations can tell us about ultimate but not proximate causes. The evolutionary programme has a limited role to play in explaining psychological phenomena. Evolutionary theory, therefore, is simply *compatible* with mainstream psychological theory.
(3) Ultimate explanations provide explanatory support for proximate explanations of psychological phenomena. The evolutionary programme will play an important role in our understanding of psychological facts. Evolutionary theory, therefore, *sublates* or *reinforces* mainstream psychological theory.
(4) Ultimate explanations explain proximate explanations. The evolutionary programme should be accepted by psychologists as the dominant theoretical

paradigm. Evolutionary theory, therefore, *incorporates* mainstream psychological theory.

Which one of these four alternatives best represents the conceptual situation in psychology will not be an all or nothing affair. Different phenomena in psychology may be best understood in terms of any of these four different positions on the relationship between ultimate and proximate explanations.

Generally speaking, however, position one and two are unlikely to be tenable stances to take in psychology. Although I do not reject the possibility that proximate causes can screen-off ultimate ones, it is unlikely that this will typically be the case. For example, in explaining the ritualised homosexuality (H) found in Melanesian cultures, we may want to primarily invoke proximate causes, in terms of the beliefs prevalent in the community (C). We could also provide ultimate explanations of just why these beliefs happen to be present in these cultures. We may want to invoke specific social learning mechanisms to account for these beliefs and discuss their fit with evolved psychological mechanisms (E). The question here is: does C screen off E from X ? That is, do socio-cultural explanations make statistically irrelevant evolutionary explanations in furthering our understanding of ritualised homosexuality in Melanesia?

I would argue that this cannot be the case. The probability of some pattern of belief cannot be independent of how those beliefs came about, unless those beliefs are entirely arbitrary and free-floating. However, this is unlikely to be the case, given the sorts of reasons outlined in chapters three and eight regarding the importance of conceptualising learning as in some sense framed in nature. That is, learning in general and social learning in particular must be organised along lines that are relevant to the organism.

In general, it is important to note that the different levels of temporal and spatial analysis should not be conceived of in terms of an intellectual division of labour. That is, although some specialisation is inevitable, it is clear that explanations drawn at different levels are relevant to one another in important ways. The distinction drawn in chapter one between explanation as unification and explanation as elucidation of mechanisms, I believe, is helpful in this context. The evolutionary programme offers a 'top-down' approach to explaining psychological phenomena. That is, psychological and social facts are viewed as instances of more general regularities. More specifically, psychological phenomena are viewed as adaptations or the products of adaptations. Mainstream psychological theories, by contrast, typically adopt a bottom-up approach. Generally speaking, they are concerned with explaining the specific mechanisms which underlie individual events.

In terms of this view of explanation, bottom-up explanations can be viewed as instantiations of top-down processes. Proximate mechanisms, therefore, are specific instances of more general regularities. Of course it is always possible to proceed in an elucidation of proximate causal mechanisms without reference to ultimate causes, but it is not necessarily desirable. A consideration of ultimate function provides, firstly, some clues as to the sorts of mechanisms that might have evolved to solve the relevant adaptive problems (Tooby & Cosmides, 1992). Of course, any given function can be realised in multiple ways, so knowing the function of some item will not necessarily lay bare its mechanism. However, a consideration of function, coupled with knowledge about the phylogenetic history of the organism will provide constraints on the sorts of processes that could have evolved to solve specific adaptive problems. In evaluating the various theories of proximate mechanisms that psychologists develop, therefore, one important criterion will be the theory's fit with what is known about our evolutionary history, both in terms of the specific ecological conditions under which humans evolved and in terms of the antecedent psychological mechanisms that our ancestors were likely to have possessed.

Given these points, however, ultimate explanations will not always, or perhaps even typically explain proximate ones. Firstly, as noted above, proximate explanations are underdetermined by ultimate ones. Secondly, there is the possibility that some important mechanisms, especially social ones, are emergent. That is, they cannot be conceptualized as adaptations, nor do they map onto adaptations in any clear-cut manner.

The foregoing analysis suggests that the relationship between the evolutionary programme and other kinds of explanation in psychology is one of sublation. Evolutionary theory provides reinforcement to, or a rationale for, the specific theories typically developed by psychologists. This would suggest that the evolutionary programme does not offer the promise of conceptual revolution for psychology. However, I think it is also clear that in furthering our understanding of many psychological phenomena we need to draw on the full resources of the evolutionary research programme. To explain, for example, humans' capacity to attribute mental states to others, we need to understand the functional significance of the relevant underlying mechanisms and the ecological and phylogenetic contexts in which they evolved (Baron-Cohen, 1995). Models of the specific psychological mechanisms underlying this capacity certainly need to be consistent with the facts about human evolution, but are not entirely explained by them.

Explanatory relevance and explanatory salience

I would suggest that evolutionary explanations are typically, if not always, explanatorily *relevant* in furthering our understanding of some psychological phenomena, but will differ greatly in terms of their explanatory *salience*. Recalling Railton's (1981) distinction drawn in chapter one, we can consider that one central goal of science is to provide us with the ideal explanatory text, while specific scientific endeavors are often aimed at gleaning explanatory information about the world. The ideal explanatory text, as Salmon (1989) notes, determines what counts as explanatorily relevant. What is salient, however, will depend on specific features of the individual and the kinds of question that they ask of the world.

Many of the sorts of questions that psychologists ask focus attention on specific classes of causes. Typically speaking, requests for information are directed towards specific features of the intra-personal, social, and cultural environment. Psychologists are often concerned with the causes of individual differences located in particular patterns of development or in term of socialization to different cultural contexts. To use a simple example, we may ask the question: "Why does Kengo speak Japanese whereas Bob speaks English?" Our answer to this question is simple: Kengo was raised in an environment of Japanese speakers whereas Bob was exposed to an environment in which English was the main language. This answer makes no reference to evolutionary theory at all. Evolutionary theory is not salient in this case. It is, however, explanatorily relevant, for it provides the ultimate explanation of how it comes to be that Kengo and Bob happen to be capable of learning a language, by referring to the selection processes which have led to the evolution of an innate universal language acquisition device in humans.

This evolutionary explanation becomes salient if we ask a slightly different question: "Why does Bob, but not Bob's pet frog Frodo, speak English? Our explanation in this case will focus on the different evolutionary histories of Bob and Frodo, and how in one case this had led to the development of a language acquisition device, and in the other it has not. A schematic representation of these different kinds of explanation is presented in figure twelve below.

Figure twelve **Explanation seeking why questions in psychology and the salience of different kinds of explanation**

Ultimate explanation	Proximate psychological explanation	Proximate social explanation	Explanation seeking why question.
The ancestors of modern humans evolved a language acquisition device which has the function of communication of propositional knowledge over a serial channel.	She possesses a language acquisition device.	She was raised in a social environment of Japanese speakers.	Why does Kengo speak Japanese?
	He possesses a Language acquisition device.	He was raised in a social environment of English speakers.	Why does Bob speak English?
The ancestors of modern frogs underwent no selection for the development of language acquisition devices.	He does not possess a language acquisition device.	He was raised in an environment of English speakers.	Why does Frodo the frog not speak English?

It is clear, therefore, that evolutionary explanations are relevant in furthering our understanding of psychological phenomena, but are they usually salient? Consider another example that I have discussed in the previous chapter: the widespread practice of infibulation in northeastern African societies. Evolutionary psychologists (e.g., Wilson & Daly, 1992) use this example as an instance of the evolved male sexual jealousy mechanism in operation. However, the invocation of this mechanism, although relevant is not necessarily salient. To explain the practice of infibulation we need to primarily advert to the specific beliefs held by men and women in these cultures and to the environment that they inhabit. In this context we need to discuss the role of Islam, the limited autonomy and status of women generally, the cultural necessity of infibulation for marriage, and the importance of marriage as the only way of women obtaining any kind of status. To be sure, the kinds of beliefs held by men are surely influenced to a considerable degree by their relevant evolved psychological mechanisms. However, reference to male sexual jealousy in this case only furthers our understanding of infibulation so far, and fails to fully explain why infibulation is practiced in some cultures but not others.

Buss (1992, p. 2) argues that "all psychological theories, even the most ardently environmental ones, imply the existence of psychological mechanisms." Buss argues further that such psychological mechanisms are best considered as evolved adaptations with distinct evolutionary histories. Buss is surely correct here. At some stage our explanations of psychological and social phenomena must invoke psychological mechanisms. These mechanisms, moreover, will typically if not always, be the product of evolutionary forces. Evolutionary theory, therefore, will be relevant in all explanations in psychology. However, the salience of evolutionary theory will vary from case to case. I have somewhat laboured this point, but I think it is an important one and serves to explain why psychologists are typically reluctant to invoke evolutionary theory in explaining the phenomena that they study. Having said this, I would suggest that evolutionary theory will often be salient in providing adequate explanations for psychological phenomena. As my discussion of jealousy in the last chapter suggests, many of the phenomena associated with jealousy, as well as the specific mechanisms underlying jealousy, can be explained by the various relevant theories of the evolutionary programme. This I think will be the case in many areas of psychology. The evolutionary programme becomes less salient, however, in explaining sociological or economic phenomena, and is virtually irrelevant in the development of theories of literature.

The evolutionary programme and the future of psychology

The foregoing discussion suggests that there will be no revolution in psychology. No effigies of Skinner, Piaget, or Freud will be burned, and portraits of Darwin will not become mandatory on the walls of psychologists' offices. Psychology is not, and will never be, an entirely autonomous science. However, it is unlikely that it will be subsumed by biology. The evolutionary programme sublates rather than incorporates or replaces existing psychological theory. However, in our attempts to unravel the ideal explanatory text, psychologists must draw on the full resources of the evolutionary research programme. In this final section I shall discuss some of the ways whereby the evolutionary research programme might further progress in psychology.

The evolutionary research programme offers, firstly, a means of furthering conceptual progress in psychology. By invoking the concept of proper function, psychologists have a potentially powerful tool in improving their categorisation schemes of psychological phenomena. By carving the mind at its functional joints, we are likely to produce more realistic orderings of phenomena and more clearly demarcate information from noise in the way that we address the subject matter of psychology.

The evolutionary programme also offers psychologists a way of making explanatory progress. The use of evolutionary theory helps us to further understand the nature of psychological phenomena and provides a means of integrating causal-mechanistic explanations with unifying ones. As the evolutionary programme in psychology develops it should provide us with more epistemically acceptable adaptation explanations of psychological phenomena and extend the scope of such explanations to new phenomena. The development of plausible group selection explanations, for example, may further our understanding of some social and moral phenomena of interest to psychologists (e.g., Wilson, 1997). In general, the evolutionary programme offers to bring some kind of conceptual integration to psychology. The phenomena that psychologists study and the mechanistic explanations that they develop will be typically conceptualised as the product of evolutionary processes, and evaluation of alternative proximal theories will be fruitfully undertaken with respect to what we know of human evolution.

Organisational progresses can also be furthered in psychology by acceptance of a more prominent role for the evolutionary programme. The relations between different areas of psychology and between psychology and the other sciences can be improved by a more careful consideration of the role of the evolutionary programme in psychology. For example, theories of social learning need to be related to what we know about the evolution of our species and the adaptive functions of the mind. The realisation that learning is unlikely, in general, to be the result of general purpose mechanisms but instead reflects domain-specific processes should further our understanding of how children come to understand the world. Such considerations improve the external coherence of both the evolutionary research programme and those specific theories of social learning.

For the acceptance of the evolutionary programme in psychology to become more widespread there also needs to be a careful consideration of the institutional features of the programme and the way that the programme relates to society. This is also true of psychology more generally speaking. It is unlikely that the day to day practices of psychologists will change to any considerable degree, even if the evolutionary programme plays a more prominent role in psychology then it has in the past. As LeCerra and Kurzbin (1995, p. 4) note, ". . . all psychologists need not be evolutionary psychologists *per se*, just as all biologists need not be evolutionary biologists studying the principles of evolution." However, some institutional features of psychology will change if the evolutionary programme becomes more widely embraced. Importantly, the education of psychologists would be altered so as to include a more prominent discussion of the nature of evolutionary theory and its implications for psychology. A more prominent role for comparative and evolutionary psychology in the education of psychologists is likely to clarify the role of evolutionary explanations in the social sciences for a greater number of psychologists. It should also serve to reduce some of the misunderstandings demonstrated by psychologists in their evaluation of the evolutionary programme.

I also believe there needs to be progress made in terms of the relations between he evolutionary programme, psychology and society. Evolutionary explanations are often tarred with the brush of determinism and are viewed as providing a scientific crutch for propping up the status quo in society and justifying extant inequalities. As I have discussed in chapter five, these beliefs are neither consistent with evolutionary theory, nor are they justifiable ethically speaking. Because our best understanding of human nature potentially exerts a profound effect on social policy and human action, more attention needs to be paid to interface between the evolutionary programme and society.

Ultimately the evolutionary programme offers to further progress the field of psychology by helping to provide richer and more realistic models of the world. By providing a deeper understanding of ourselves, our relations to other species, and the world that we live in, we can engage in scientific inquiry in a way commensurate with the goal of producing valuable knowledge.

References

Aiello, L. C. (1996). Terrestriality, Bipedalism and the origin of language. In W. G. Runciman, J. Maynard-Smith, & R. I. M. Dunbar (Eds.). *Evolution of social behaviour patterns in primates and man.* (pp269-291). Oxford: Oxford University Press.

Alexander, R, D. (1979). *Darwinism and human affairs*. Seattle: University of Washington Press.

Alexander, R. D. (1990). Epigenetic rules and Darwinian algorithms: The adaptive study of learning and development. *Ethology and Sociobiology, 11,* 241-303.

Allen, E. (1978). Against sociobiology. In A. L. Caplan (Ed.). *The sociobiology debate* (pp 259-264). New York: Harper & Row.

Anderson, J. R. (1983). *The architecture of cognition.* Cambridge, MA: Harvard University Press.

Atkinson, R. L., Atkinson, R. C., Smith, E. E., Bem, D. J., Nolen-Hoeksema, S. (1996). *Hilgard's introduction to psychology.* Fort Worth: Harcourt Brace College Publishers.

Atran, S. (1990). *Cognitive foundations of natural history.* New York: Cambridge University Press.

Baker, R. R. & Bellis, M. A. (1989). Number of sperm in human ejaculates varies in accordance with sperm competition theory. *Animal Behaviour, 37* (5), 867-869.

Baker, R. R. & Bellis, M. A. (1990). Variation in rat ejaculates consistent with the Kamikaze-sperm hypothesis. *Journal of Mammalogy, 71* (3), 479-480.

Baker, R. R. & Bellis, M. A. (1995). *Human sperm competition*. New York: Harcourt Press.

Barash, D. P. (1982). *Sociobiology and behavior*. New York: Elsvier.

Baron-Cohen, S. (1995). *Mindblindness: An essay on autism and theory of mind.* Cambridge, Massachusetts: MIT Press

Barkow, J. H. (1989). *Darwin, sex, and status: Biological approaches to mind and culture.* Toronto: University of Toronto Press.

Beach, F. A. (1955). The descent of instinct. *Psychological Review, 62* (6), 401-410.

Beach, F. A. (1965). The snark was a boojum. In T. E. McGill (Ed.). *Readings in animal behaviour*. New York: Holt, Rinhart, and Winston.

Betzig, L. (1989). Causes of conjugal dissolution: a cross-cultural study. *Current Anthropology, 30*, 654-676.

Betzig, L. L. & Turke, P. W. (1986). Food sharing on Ifaluk. *Current Anthropology, 27* (4), 397-400.

Bevan, W. (1991). Contemporary psychology: A tour inside the onion. *American Psychologist, 46* (5), 475-483.

Bigelow, J. & Pargetter, R. (1987). Functions. *Journal of philosophy, 84* (4), 181-196.

Bitterman, M. E. (1960). Toward a comparative psychology of learning. *American Psychologist*, 705-713.
Blurton Jones N. G. (1990). Three sensible paradigms for research on evolution and human behavior. *Ethology and Sociobiology*, 11, 353-359.
Bloor, D. (1981). The strengths of the strong programme. *Philosophy of Social Science, 11*, 173-198.
Bouskila, A., and Blumstein, D. T. (1992). Rules of thumb for predation hazard assessment: predictions from a dynamic model. *The American Naturalist, 139* (1), 161-176.
Boesch, C. (1991). Teaching in wild chimpanzees. *Animal behaviour, 41* (3), 530-532.
Boesch, C. (1996). The emergence of cultures among wild chimpanzees. In W. G. Runciman, J. Maynard-Smith, & R. I. M. Dunbar (Eds.). *Evolution of social behaviour patterns in primates and man.* Oxford: Oxford University Press.
Bogen, J. & Woodward, J. (1988). Saving the phenomena. *The Philosophical Review, 97*, 303-353.
Borgerhoff Mulder, M. (1987). Adaptation and evolutionary approaches to anthropology. *Man, 22* (1), 25-42.
Borgerhoff Mulder, M. (1988). Kipsigis bridewealth payments. In L. L. Betzig, M. Borgerhoff Mulder, & P. W. Turke (Eds.). *Human reproductive behavior.* Pp 65-82 Cambridge: Cambridge University Press.
Borgerhoff Mulder, M. (1991). Human behavioral ecology. In J. R. Krebs & N. D. Davies (Eds.). *Behavioural ecology: An evolutionary approach,* 3rd edition. London: Blackwell.
Borgerhoff Mulder, M. (1996). Responses too environmental novelty: Changes in men's marriage strategies in a rural Kenyan community. In W. G. Runciman, J. Maynard Smith, & R. I. M. Dunbar (Eds.). *Evolution of social behavior patterns in primates and man.* Oxford: Oxford University Press.
Boyd, R. N. (1984). The current status of scientific realism. In J. Leplin (Ed.). *Scientific realism.* Berkeley: University of California Press. Pp 41-83.
Boyd, R. & Richerson, P. J. (1985). *Culture and the evolutionary process.* Chicago: University of Chicago Press.
Brandon, R. N. (1990). *Adaptation and environment.* Princeton, New Jersey: Princeton University Press.
Brewer, M. B., & Caporael, L. R. (1990). Selfish genes vs. Selfish people: Sociobiology as origin myth. *Motivation & Emotion*, 237-243.
Breland, K & Breland, M. (1961). The misbehavior of organisms. *American Psychologist, 16*, 681-684.
Bringle, R. G., & Buunk, B. (1985). Jealousy and social behaviour. *Review of Personality and Social Psychology, 6*, 241-264.
Brown, D. E, (1991). *Human Universals.* New York: McGraw-Hill.
Bunge, M. (1990). What kind of discipline is psychology: Autonomous or dependent, humanistic or scientific, biological or sociological? *New Ideas in Psychology, 8* (2), 121-137.

Bunge, M. (1996). Mechanism and Explanation. *Philosophy of the Social Sciences, 27,* (4), 410-466.

Buss, D. M. (1988). From vigilance to violence: mate guarding tactics. *Ethology & Sociobiology, 9,* 291-317.

Buss, D. M. (1989). Conflict between the sexes: strategic interference and the evocation of anger and upset. *Journal of Personality and Social Psychology,* 56, 735-747.

Buss, D. M. (1995). Evolutionary psychology: A new paradigm for psychological science. *Psychological Inquiry, 6* (1), 1-30.

Buss, D. M. (1994). *The evolution of desire: strategies of human mating.* New York: Basic Books.

Buss, D. M., & Barnes, M. F. (1986). Preferences in human mate selection. *Journal of Personality and Social Psychology, 50,* 559-570.

Buss, D. M. & Schmitt, D. P. (1993). Sexual strategies theory: an evolutionary perspective on human mating. *Psychological Review, 100,* 204-232.

Buss, D. M., Larsen, R. J., Westen, D., & Semmelroth, J. (1992). Sex differences in jealousy: evolution, physiology, and psychology. *Psychological Science, 3,* 251-255.

Butterworth, G. (1991). The ontogeny and phylogeny of joint visual attention. In A. Whiten (Ed.). *Natural theories of mind: Evolution, development and simulation of everyday mindreading.* Oxford: Basil Blackwell

Buunk, B. (1982). Strategies of jealousy: styles of coping extra-marital involvement of the spouse. *Family Relations, 31,* 13-18.

Buunk, B. (1991). Jealousy in close relationships: an exchange-theoretical perspective. In P. Salovey (Ed.). *The psychology of jealousy and envy.* New York: The Guilford Press.

Buunk, B., & Hupka, R. B. (1987). Cross-cultural differences in the elicitation of sexual jealousy. *Journal of Sex Research, 23,* 12-22.

Buunk, B., Angleitner, A., Oubaid, V., & Buss, D. B. (1996). Sex differences in jealousy in evolutionary and cultural perspective: tests from the Netherlands, Germany, and the United States. *Psychological Science, 7* (6), 359-363.

Byrne, D. (1996). *The Thinking Ape: Evolutionary origins of intelligence.* Oxford: Oxford University Press.

Calvin, W. (1993). The unitary hypothesis: A common neural ccircuitry for novel manipulation, language, plan-ahead, and throwing? In K. R. Gibson, & T. Ingold (Eds.). *Tools, Language and cognition in human evolution.* Cambridge: Cambridge University Press.

Campbell, D. T. (1982). Evolutionary epistemology. In H. C. Plotkin (Ed.). *Learning and development: essays in evolutionary epistemology.* Chichester: John Wiley & Sons.

Cantor, N. (1990). Social psychology and sociobiology: what can we leave to evolution? *Motivation and Emotion, 14* (4), 245-254.

Caplan, A. L. (1989). Say it just ain't so: Adaptational stories and sociobiological explanations of social behaviour. In M. Ruse (Ed.). *Philosophy of biology.* New York: Macmillan Publishing company.

Caporael, L. R. & Brewer, M. B. (1991). Reviving evolutionary psychology: biology meets society. *Journal of social issues, 47,* (3), 187-195.

Caporael, L. R. & Baron, R. M. (1997). Group's as the mind's natural environment. In J. A. Simpson & D. T. Kenrick (Eds.). *Evolutionary social psychology.* (pp 317-345)New Jersey : Lawrence Erlbaum Associates.

Carey, S. and Gelman, R. (Eds.). (1991). *The epigenesis of mind: essays on biology and cognition.* New Jersey: Lawrence Erlbaum Associates.

Caro, T. M. & Borgerhoff-Mulder, M. (1987). The problem of adaptation in the study of human behavior. *Ethology and Sociobiology, 8,* 61-72.

Chagnon, N. A. (1988). Life histories, blood revenge, and warfare in a tribal population. *Science, 239,* 985-992.

Chagnon, N. & Irons, W. G. (Eds.). (1979). *Evolutionary biology and human social behavior: An anthropological perspective.* North Scituate, Massachusetts: Duxbury.

Chalmers, A. F. (1982). *What is this thing called science?* St. Lucia: University of Queensland Press.

Chalmers, A. F. (1990). *Science and its fabrication.* Brisbane: University of Queensland Press.

Cheney, D. L., & Seyfarth, R. M. (1990). *How monkey's see the world: inside the mind of another species.* Chicago: Chicago University Press.

Cherry, P. W., & Holyoak, K. J. (1989). On the natural selection of reasoning theories. *Cognition, 35* (3), 285-313.

Chomsky, N. (1959). Review of, *Verbal Behavior, Language, 35* (1), 26-58.

Chomsky, N. (1972). *Language and mind.* New York: Harcourt Brace.

Churchland, P. M. (1981). Eliminative materialism and the propositional attitudes. *Journal of Philosophy, 78,* 67-90.

Churchland, P. M. (1985). Reduction, qualia, and the direct introspection of brain states. *Journal of Philosophy, 82,* 8-28.

Churchland, P. M. (1989). *A neurocomputational perspective: the nature of mind* and *the structure of science.* Cambridge, MA: MIT Press.

Clark, A. (1989). *Microcognition: philosophy, cognitve science and parallel distributed processing.* Cambridge, MA: MIT Press.

Clutton-Brock, T. H. (Ed.). (1988). *Reproductive success: studies of individual variation in contrasting breeding systems.* Chicago: University of Chicago Press.

Coddington, J. A. (1988). Cladistic tests of adaptational hypotheses. *Cladistics, 4,* 3-22.

Corballis, M. C. (1990). The two profiles of psychology. *New Zealand Journal of Psychology, 19,* 2-8.

Corballis, M. (1991). *The lopsided ape.* New York: Oxford University Press.

Cosmides, L. and Tooby, J. (1987). From evolution to behaviour: Evolutionary psychology as the missing link. In J. Dupre (Ed.). *The latest on the best: essays on evolution and optimality.* Cambridge, Massachusetts: MIT Press.

Cosmides, L. & Tooby, J. (1989). Evolutionary psychology and the generation of culture, part II: Case study: A computational theory of social exchange. *Ethology and Sociobiology, 10,* 51-89.

Cosmides, L. and Tooby, J. (1994). Origins of domain specificity: the evolution of functional organisation. In L. A. Hirschfeld & S. A. Gelman (Eds.). *Mapping the mind: domain specificity in cognition and culture.* Cambridge: Cambridge University Press.

Crisp, R. (1996). Evolution and psychological unity. In M. Beckoff, & D. Jamieson (Eds.). *Readings in animal cognition.* Cambridge, Mass: MIT Press.

Cronk, L. (1991). Human behavioral ecology. *Annual Review of Anthropology, 20,* 25-53.

Crook, J. H. & Crook, S. J. (1988). Tibetan polyandry: Problems of adaptation and fitness. In L. Bezig, M. Borgerhoff Mulder, & P. Turke (Eds.). *Human reproductive behavior.* Cambridge: Cambridge University Press.

Daly, M., & Wilson, M. (1988). *Homicide.* New York: Aldine de Gruyter.

Daly, M., Wilson, M., & Weghorst (1982). Male sexual jealousy. *Ethology & Sociobiology, 3,* 11-27.

Darwin, C. (1859). *The origin of species by means of natural selection.* New York: Random House.

Darwin, C. (1871). *The Descent of man and selection in relation to sex.* New York: Random House.

Davidson, I., & Noble, W. (1989). The archaeology of perception: Traces of depiction and language. *Current Anthropology, 30* (2), 125-137.

Davies, N. B. (1986). Reproductive success of dunnocks, *Prunella modularis,* in a variable mating system. *Journal of Animal Ecology, 55,* 123-138.

Dawkins, R. (1976). *The selfish gene.* Oxford: Oxford University Press.

Dawkins, R. (1982). *The extended phenotype.* Oxford: Oxford University Press.

Dawkins, R. (1983). Universal Darwinism. In D. S. Bendall (Ed.). *Evolution from molecules to men.* Cambridge: Cambridge University Press.

Dawkins, R. (1986). *The blind watchmaker.* Oxford: Penguin.

Dawkins, R. (1996). *Climbing mount improbable.* London: Viking Press.

Degler, C. N. (1991). *In search of human nature: The decline and revival of Darwinism in American social thought.* NewYork: Oxford University Press.

Demarest, J. (1987). On changing the framework of psychology: comparative psychology is what general psychology should be. *Teaching Psychology, 14 (3),* 147-150.

Dennett, D. C. (1978). *Brainstorms.* Cambridge, MA: MIT Press.

Dennett, D. C. (1981). Three kinds of intentional psychology. In R. Healey (Ed.). *Reduction, time, and reality.* New York: Cambridge University Press.

Dennett, D. C. (1983). Intentional systems in cognitive ethology: the Panglossian paradigm defended. *Behavioural and brain sciences, 6,* 343-391.

Dennett, D. C. (1995). *Darwin's dangerous idea: evolution and the meanings of life.* New York: Simon & Schuster.
Desmond, A., & Moore, J. (1991). *Darwin.* London: Penguin Books.
DeSteno, D. A., & Salovey, P. (1994). Jealousy in close relationships: multiple perspectives on the green-ey'd monster. In A. L. Weber and J. H. Harvey (Eds.). *Perspectives on close relationships.* Boston: Allyn and Bacon.
Dixon, R. A. (1983). Theoretical proliferation in psychology: A plea for sustained disunity. *Psychological Record, 33,* 337-340.
Dobzhansky, T. (1973). Nothing in biology makes sense except in the light of evolution. *American Biology Teacher, 35,* 125-129.
Donald, M. (1991). *Origins of the modern mind: three stages in the evolution of culture and cognition.* Cambridge, MA: Harvard University Press.
Doolittle, W. F. & Sapienza, C. (1980). Selfish genes, the phenotype paradigm, and genome evolution. *Nature, 284,* 601-603.
Dunbar, R. I. M. (1993). Co-evolution of neocortex size, group size, and language in humans. *Behavioural and Brain Sciences, 16,* 681-735.
Dunbar, R. I. M. (1995). *The trouble with science.* London: Faber & Faber.
Dunbar, R. I. M. (1996). *Grooming, gossip, and the evolution of language.* London: Faber & Faber.
Dupre, J. (1993).*The disorder of things: metaphysical foundations of the disunity of science.* Cambridge: Cambridge University Press.
Dupre, J. (1996). The mental lives of non-human animals. In M. Beckoff, & D. Jamieson (Eds.). *Readings in animal cognition.* Cambridge, Mass: MIT Press.
Edelman, G. E. (1989). *The remembered present: a biological theory of consciousness.* New York: Basic Books.
Ekman, P. (1992). An argument for basic emotions. *Cognition and Emotion, 6,* 169-200.
Eldredge, N. (1995). *Reinventing Darwin: the great debate at the high table of evolutionary theory.* New York: John Wiley and sons Inc.
Feyerabend, P. (1975). How to defend society against science. *Radical Philosophy, 11,* 3-8.
Fisher, J. A. (1991). Disambiguating anthropomorphism: An interdisciplinary review. *Perspectives in Ethology, 9,* 49-85.
Flanagan, O. J. (1991). *The science of the mind.* Cambridge, MA: MIT Press.
Fletcher, G. J. O. (1995). *The scientific credibility of folk psychology.* Hillsdale, NJ: Lawrence Erlbaum.
Flinn, M. (1988). Mate guarding in a Caribbean village. *Ethology and Sociobiology, 9,* 1-28.
Flynn, J. R. (1987). Massive IQ gains in 14 nations: What IQ tests really measure. *Psychological Bulletin, 101,* 171-191.
Fodor, J. (1983). *The modularity of mind.* Cambridge, MA : MIT Press.
Fodor, J. (1984). Observation reconsidered. *Philosophy of Science, 51,* 23-43.

Fodor, J. (1991). Methodological solipsism considered as a research strategy in cognitive psychology. In R. Boyd, P. Gasper, & J. D. Trout. (Eds.). *The philosophy of science*. Cambridge, Massachusetts: MIT Press.

Foley, R. A. (1987). *Another unique species: Patterns in human evolutionary ecology*. London: Longman.

Foley, R. A. (1996). An evolutionary and chronological framework for human social behaviour. In W. G. Runciman, J. Maynard-Smith & R. I. M. Dunbar (Eds.). *Evolution of social behaviour patterns in primates and man* (pp95-119). Oxford: Oxford University Press.

Foley, R. A. & Lee, P. C. (1989). Finite social space, evolutionary pathways and reconstructing hominid behaviour. *Science, 243*, 901-906.

Ford, C. S., & Beach, F. A. (1952). *Patterns of sexual behaviour*. New York: Harper & Row.

Friedman, M. (1974). Explanation and scientific understanding. *Journal of Philosophy, 71*, 5-19.

Friman, P. C., Allen, K. D., Kerwin, M. L., & Larzleler, R. (1993). Changes in modern psychology: A citation analysis of the Kuhnian displacement thesis. *American Psychologist, 48*, 658-664.

Gangstead, S. W., & Simpson, J. A. (1990). Towards an evolutionary history of female socio-sexual variation. *Journal of Personality, 58*, 69-96.

Gangstead, S. W. & Thornhill, R. (1997). The evolutionary psychology of extra-pair sex: the role of fluctuating asymmetry. *Evolution and Human Behavior, 18*, 69-98.

Garcia, J. & Koelling, R. A. (1972). Relation of cue to consequence in avoidance learning. In M. E. P. Seligman & J. L. Hager (Eds.). *Biological boundaries of learning*. pp 10-15. New York: Appleton - Century - Crofts.

Garcia, J., Ervin, F. R., & Koelling, R. A. (1972). Very long CS - US intervals in taste aversion learning. In M. E. P. Seligman & J. L. Hager (Eds.). *Biological boundaries of learning*. New York: Century - Appleton - Crofts.

Gardner, H. (1983). *Frames of mind: the theory of multiple intelligences*. New York: Basic books.

Gardner, H. (1995). Perennial antinomies and perpetual redrawings: is there progress in the study of the mind? In R. L. Solso & D. W. Massaro (Eds.). *The science of the mind: 2001 and beyond*. New York: Oxford University Press.

Gaulin, S. J. C. & Hoffman, H. A. (1988). Evolution and development of sex differences in spatial ability. In L. Betzig, M. B. Mulder, & P. Turke (Eds.). *Human reproductive behaviour: A Darwinian perspective* (pp129-152). Cambridge: Cambridge University Press.

Gazzaniga, M. S. (1992). *Nautre's mind*. New York: Basic.

Geary, D. C., Rumsey, M., Bow-Thomas, C. C., & Hoard, M. K. (1995). Sexual jealousy as a faculatative trait: evidence from the pattern of sex differences in adults from China and he United States. *Ethology & Sociobiology, 16*, 355-383.

Gelman, R. (1990). First principles organize attention to and learning about relevant data: Number and the animate-inanimate distinction as examples. *Cognitive Science, 14,* 79-106.

Gergen, K. J. (1985). The social constructionist movement in modern psychology. *American Psychologist, 40,* 266-275.

Gergen, K. J. (1992). Toward a postmodern psychology. In S. Kvale (Ed.). Psychology and postmodernism. London: Sage Publications.

Gholson, B. & Barker, P. (1985). Kuhn, Lakatos, and Laudan: applications in the history of physics and psychology. *American Psychologist, 40,* 755-769.

Gigerenzer, G. & Hug, K. (1992). Domain-specific reasoining: Social contracts, cheating, and perspective change. *Cognition, 43,* 127-171.

Giere, R. N. (1988). *Explaining Science: A cognitive approach.* Chicago: University of Chicago Press.

Giorgi, A. (1992). Toward the articulation of psychology as a coherent discipline. In S. Koch & D. E. Leary (Eds.). *A century of psychology as a science.* Washington: American Psychological Association.

Goodall, J. (1986). *The chimpanzees of Gombe: patterns of behavior.* Cambridge, MA: MIT Press.

Gopnik, M., Dalalakis, J., Fekuda, S. E., Fekuda, S., & Kehayia, E. (1996). Genetic language impairments: Unruly grammars. In. W. G. Runciman, J. Maynard-Smith, & R. I. M. Dunbar (Eds.). *Evolution of social behaviour patterns in primates and man.* (pp 223-251). Oxford: Oxford University Press.

Gould, S. J. (1980a). The panda's thumb. *In The panda's thumb.* London: Penguin Books.

Gould, S. J. (1980b). Senseless signs of history. In *The panda's thumb.* London: Penguin Books.

Gould, S. J. (1980c). Natural selection and the human brain: Darwin vs. Wallace. In *The panda's thumb.* London: Penguin Books.

Gould, S. J. (1981). *The mismeasure of man.* New York: Norton.

Gould, S. J. (1985). Singapore's patrimony (and matrimony). In *The flamingo's smile.* New York: W. W. Norton.

Gould, S. J. (1985). *The flamingo's smile.* New York: W. W. Norton.

Gould, S. J. (1989a). Sociobiology and the theory of natural selection. In M. Ruse (Ed.). *Philosophy of biology.* New York: Macmillan Publishing Company.

Gould, S. J. (1989b). *Wonderful life: The Burgess Shale and the nature of history.* New York: W. W. Norton.

Gould, S. J. (1989c). Darwinism and the expansion of evolutionary theory. In M. Ruse (Ed.). *Philosophy of biology.* New York: Macmillan.

Gould, S. J. (1989d). Tires to sandals. *Natural History,* April, 8-15.

Gould, S. J. (1991a). Of kiwi eggs and the liberty bell. In *Bully for brontosaurus.* London: Penguin Books.

Gould, S. J. (1991b). Male nipples and clitoral ripples. In *Bully for brontosaurus.* London: Penguin Books.

Gould, S. J. (1991c). Exaptation: A crucial tool for an evolutionary psychology. *Journal of Social Issues, 47* (3), 43-65.

Gould, S. J. (1991d). The panda's thumb of technology. *In Bully for Brontosaurus.* London: Penguin

Gould, S. J. (1991). Kropotkin was no crackpot. In *Bully for brontosaurus.* London: Penguin Books.

Gould, S. J. & Lewontin, R. C. (1979). The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme. *Proceedings of the Royal Society of London, B 205,* 581-598.

Gould, S. J. & Vrba, E. S. (1982). Exaptation-a missing term in the science of form. *Paleobiology, 8,* 4-15.

Greenwood, J. D. (1982). On the relations between laboratory experiments and social behaviour: causal explanation and generalization. *Journal for the Theory of Social Behaviour, 12* (3), 225-250.

Greenwood, J. D. (1987). A realist science of psychology. In J. D. Greenwood (Ed.). *The idea of psychology: conceptual and methodological issues.* Singapore: Singapore University Press.

Greenwood, J. D. (1992). Realism, empiricism and social constructionism: psycholgical theory and the social dimensions of mind and action. *Theory and Psychology,* 2 (2), 131-151.

Greenwood, R. M., & Atkinson, I. A. E. (1977). Evolution of divaricating plants in New Zealand in relation to moa browsing. *Proceedings of the New Zealand Ecological Society, 24,* 21-33.

Griffin, D. R. (1978). *The question of animal awareness: evolutionary continuity of mental experience.* New York

Griffin, D. R. (1984). *Animal thinking.* Cambridge: Harvard University Press.

Griffiths, P. (1992). Adaptive explanation and the concept of a vestige. In P. Griffiths (Ed.). *Trees of life.* Dordrecht: Kluwer Academic Publishers.

Griffiths, P. (1992). Death of the gene: developmental systems strike back. In P. Griffiths (Ed.). *Trees of life.* Dordrecht: Kluwer Academic Publishers.

Griffiths P. E. & Gray, R. D. (1994). Developmental systems and evolutioanry explanation. *The journal of Philosophy, 91* (6), 277-304.

Grunbaum, A. (1986). Precis of the foundations of psychoanalysis. *Behavioural and Brain Sciences, 9* (2), 217-228.

Hamilton, W. D. (1964a). The genetical evolution of social behaviour, I. *Journal of Theoretical Biology, 7,* 1-16.

Hamilton, W. D. (1964b). The genetical evolution of social behaviour II. *Journal of Theoretical Biology, 7,* 17-32.

Hamilton, W. D., & Zuk, M. (1982). Heritable true fitness and bright birds: a role for parasites. *Science, 218,* 384-387.

Harcourt, A. H., Harvey, P. H., Larson, S. G., Short, R. V. (1981). Testis weight, body weight and breeding system in primates. *Nature, 293*, 55-56.
Harris, C. R., & Christenfeld, N. (1996). Gender, jealousy, and reason. *Psychological Science, 7* (6), 364-366.
Hasegawa, M., Kishino, H, & Yano, T. (1989). Estimation of branching dates among primates by molecular clocks of nuclear DNA which slowed down in Hominoidea. *Journal of Human Evolution, 18*, 461-476.
Hazen, C., & Shaver, P. R. (1987). Love and work: An attachment-theoretical perspective. *Journal of Personality and Social Psychology, 59*, 270-280.
Hacking, I. (1984). Experimentation and scientific realism. In J. Leplin (Ed.). *Scientific realism*. Berkeley: University of California Press. Pp 154-173
Haig, B. D. (1989). Why we must accept nuclear winter theory. *Bulletin of Peace Proposals, 20* (1), 81-88.
Haig, B. D. (1990). Psychology as philosophy. *New Zealand Journal of Psychology, 19* (2), 81-83.
Hallam, D. (1983). Plate tectonics and evolution. In D. S. Bendall (Ed.). *Evolution from molecules to men*. London: Cambridge University Press.
Hamilton, W. D. (1964a). The genetical evolution of social behaviour 1. *Journal of Theoretical Biology, 7*, 1-16.
Hamilton, W. D. (1964b). The genetical evolution of social behaviour 2. *Journal of Theoretical Biology, 7*, 17-52.
Hare-Mustin, R. T., & Marecek, J. (1988). The meaning of difference: Gender theory, postmodernism, and psychology. *American Psychologist, 43* (6), 455-464.
Harman, G. (1965). The inference to the best explanation. *Philosophical Review, 74*, 88-95.
Harre, R. (1986a). *Varieties of Realism: A rationale for the natural sciences.* Oxford: Basil Blackwell.
Harre, R. (Ed.). (1986b). The social construction of emotions. Oxford: Basil Blackwell.
Harvey, P. H. and Pagel, M. D. (1991). *The comparative method in evolutionary biology*. Oxford: Oxford University Press.
Hempel, C. G. (1966). *Philosophy of natural science*. New Jersey: Prentic-Hall.
Herdt, G. (1984). *Ritualised homosexuality in Melanesia.* Berkely: University of California Press.
Herrnstein, R. J. (1974). Nature as nuture: behaviorism and the instinct doctrine. *Behaviorism, 1*, 23-52.
Herrnstein, R. J., & Murray, C. (1994). *The bell curve: intelligence and class structure in American life.* New York: Free Press.
Heyes, C. M. (1993). Imitation, culture, and cognition. *Animal Behaviour, 49*, 999-1010.
Heyes, C. M., & Galef, B. G., (Eds.). (1996). *Social learning in animals*. New York: Academic Press.

Hicks, E. K. (1993). *Infibulation: female mutilation in Islamic Northeastern Africa.* New Brunswick: Transaction Publishers.
Hill, J. (1984). Prestige and reproductive success in man. *Ethology and Sociobiology, 5,* 77-95.
Hull, D. L. (1974). *Philosophy of biological science.* Englewood Cliffs, New Jersey: Prentice-Hall Inc
Hull, D. L. (1984). Units of evolution: a metaphysical essay. In R. N. Brandon & R. M. Burian (Eds.). *Genes, organisms, populations: controversies over the units of selection.* Cambridge, Massachusetts: MIT Press.
Hull, D. L. (1988). *Science as a process.* Chicago: Chicago University Press.
Hupka, R. B. (1991). The motive for the arousal of romantic jealousy: its cultural origin, In P. Salovey (Ed.). *The psychology of jealousy and envy.* New York: The Guilford Press.
Huxley, T. (1863). *Zoological evidence as to man's place in nature.* London: Williams and Norgate.
Hooker, C. A. (1975). On global theories. *Philosophy of Science, 42,* 152-179.
Hooker, C. A. (1987). *A realistic theory of science.* New York: State University of New York Press.

Hrdy, S. B. (1986). Empathy, polyandry, and the myth of the coy female. In R. Bleir (Ed.). Feminist approaches to science. (pp 119-146) New York: Pergamon Press.
Irons, W. (1979). Natural selection, adaptation, and human social behavior. In N. A. Chagnon & W. Irons (Eds.). *Evolutionary biology and human social behaviour: An anthopological perspective.* North Scituate, Massachusetts: Duxbury Press. Pp 4-39.
Irons, W. (1979). Cultural and biological success. In N. A. Chagnon & W. Irons (Eds.). *Evolutionary biology and human social behavior: An anthropological perspective.* North Scituate, Massachusetts: Duxbury Press. Pp 257-273.
Jacob, F. (1977). Evolution and tinkering. *Science, 196,* 1161- 1166.
Jackson, F. (1982). Epiphenomenal qualia. *Philosophical quarterly, 32,* 127-136.
Jackson, F. (1986). What Mary didn't know. *The Journal of Philosophy, 83,* 291-295..
Josephson, J. R., & Josephson, S. G. (Eds.). *Abductive inference: computation, philosophy, and teaching.* Cambridge: Cambridge University Press.
Joynson, R. B. (1989). *The Burt affair.* London: Routledge and Kegan Paul.
Kaplan, H. & Hill, K. (1985). *Current Anthropology, 26* (2), 223-246.
Karmiloff-Smith, A. (1992). *Beyond modularity: A developmental perspective on cognitive science.* Cambridge, Mass: MIT Press.
Keil, F. C. (1990). Constraints on constrainsts: surveying the epigenetic landscape. *Cognitive Science, 14,* 135-168.

Kelley, H. H. (1992). Common-sense psychology and scientific psychology. *Annual Review of Psychology, 43,* 1-23.

Kelley, H. H. (1986). Personal relationships: their nature and significance. In R. Gilmour and S. Duck (Eds.). *The emerging field of personal relationships.* Hillsdale: Erlbaum.

Kennedy, J. S. (1992). *The new anthropomorphism.* Cambridge: Cambridge University Press.

Kevles, D. J. and Hood, L. (Eds.). (1992). *The code of codes: scientific and social issues in the human genome project.* London: Harvard University Press.

Kitcher, P. (1981). Explanatory unification. *Philosophy of Science, 48,* 507-531.

Kitcher, P. (1985). *Vaulting Ambition: Sociobiology and the quest for human nature.* Cambridge: MIT Press.

Kitcher, P. (1985). Two approaches to explanation. *Journal of Philosophy, 82,* 632-639.

Kitcher, P. (1988). Imitating selection. In M. Ho & S. W. Fox (Eds.). *Evolutionary processes and metaphors.* Chichester: John Wiley & Sons.

Kitcher, P. (1989). Explanatory unification and the causal structure of the world. In P. Kitcher and W. Salmon (eds.). *Scientific explanation.* Minneapolis: University of Minneapolis.

Kitcher, P. & Salmon, W. (Eds.). *Scientific explanation.* Minneapolis: Universtiy of Minneapolis.

Kitcher, P. (1990). Developmental decomposition and the future of human behavioral ecology. *Philosophy of Science, 57,* 96-117.

Kitcher, P. (1996). *The lives to come: the genetic revolution and human possibilities.* London: Harvard University Press.

Kitcher, P. (1991). Narrow taxonomy and wide functionalism. In R. Boyd, P. Gasper, & J. D. Trout. (Eds.). *The Philosophy of Science.* Cambridge, Massachusetts: MIT Press.

Kitcher, P. (1993). *The Advancement of science.* New York: Oxford University Press.

Koch, S. (1981). The nature and limits of psychological knowledge: lessons of a centry qua "science". *American Psychologist, 36* (3), 257-269.

Kuhn, D. (1989). Children and adults as intuitive scientists. *Psychological Review, 96* (4), 674-689.

Kuhn, T. S. (1962). *The structure of scientific revolutions.* Chicago: Chicago University Press.

Kuhn, T. S. (1977). *The essential tension.* Chicago: University of Chicago Press.

Knorr-Cetina, K. D. and Mulkay, M. (1983). Emerging principles in social studies of science. In *Science observed.* London: Sage Publications.

Lakatos, I. (1970). Falsfication and the methodology of scientific research programmes. In I. Lakatos and A. Musgrave (Eds.). *Criticism and the growth of knowledge.* Cambridge: Cambridge University Press.

Latour, B., & Woolgar, S. (1979). *Laboratory life: the social construction of scientific facts.*

Laudan, L. (1977). *Progress and its problems.* Berkeley: University of California Press.

Laudan, L. (1981). The pseudo-science of science? *Philosophy of social science, 11,* 173-198.
Laudan, L. (1984). A confutation of convergent realism. In J. Leplin (Ed.). *Scientific realism.* Berkeley: University of California Press.
Laudan, L. (1983). The demise of the demarcation problem. In R. Cohen and L. Laudan (Eds.). *Physics, philosophy and psychoanalysis.* Dordrecht: Reidal
Lazarus, R. S. (1991). *Emotion and adaptation.* New York: Oxford University Press.
Leahey, T. H. (1991). *A history of modern psychology.* New Jersey: Prentice Hall.
Leahey, T. H. (1992). The mythical revolutions of American psychology. *American Psychologist, 47,* 308-313.
LeCerra, P., & Kurzbin, R. (1995). The structure of scientific revolutions and the nature of the adapted mind. *Psychological Inquiry, 6* (1), 62-65.
Leplin, J. (1984). Truth and scientific progress. In J. Leplin (Ed.). *Scientific realism.* Berkeley: University of California Press. Pp 193-218.
Lehrman, D. S. (1953). A critique of Konrad Lorenz's theory of instinctive behaviour. *The Quarterly Review of Biology, 28* (4), 327-363.
Lewontin, R. C. (1970). The units of selection. *Annual Review of Ecology and Systematics, 1,* 1-18.
Lewontin, R. C. (1978). Adaptation. *Scientific American, 239* (9), 156-169.
Lewontin, R. C. (1979). Sociobiology and adaptation. *Behavioural Science, 24,* 5-14.
Lewontin, R. C. (1990). The evolution of cognition. In D. N. Osheron & E. E. Smith (Eds.). *Thinking: An introduction to cognitive science.* Cambridge, Mass: MIT Press.
Little, D. (1991). *An introduction to the philosophy of social science.* Boulder: Westview Press.
Lloyd-Morgan (1894). *An introduction to comparative psychology.* London: Scott

Lorenz, K. (1971). *Studies in animal and human behaviour vols 1 & 2.* London: Metheun & Co.
Lovejoy, C. O. (1981). The origin of man. *Science, 211,* 341-348.
Lovelock, J. (1988). *The ages of Gaia: a biography of living Earth.* Oxford: Oxford University Press.
Lumsden, C., & Wilson, E. O. (1981). *Genes, mind, and culture.* Cambridge, Massachusetts: Harvard University Press.
Lycan, W. G. (1987). *Consciousness.* Cambridge, MA: MIT Press.
Lyell, C. (1863). *The geological evidences of the aniquity of man.* London: John Murray, Albemarle street.
Lykken, D. T. (1991). What's wrong with psychology anyway? In D. Cicchetti & W. M. Grove (Eds.). *Thinking clearly about psychology vol 1: Matters of public interest.* Minneapolis: University of Minnesota Press.
Lyons, W. (1992). Intentionality and modern philosophical psychology 3. The appeal to teleology. *Philosophical Psychology, 5,* 309-326.
Maier, N. R. F., & Schneirla, T. C. (1935). *Principles of animal psychology.* New York: McGraw Hill.

Manicas, P. T. & Secord, P. F. (1983). Implications for psychology of the new philosophy of science. *American Psychologist*, 399-413.

Manicas, P. T. (1987). *A history and philosophy of the social sciences.* Oxford: Basil Blackwell.

Margolis, J. (1990). Explicating actions. In D. N. Robinson & L. P. Moss (Eds.). *Annals of theoretical psychology 6*, 39-74. New York: Plenum Press.

Marks, I. M. (1987). *Fears, phobias, and rituals.* New York: Oxford University Press.

Marler, P. (1991). The instinct to learn. In S. Carey & R. Gelman (Eds.). *The epigenesis of mind: essays on biology and cognition.* New Jersey: Lawrence Erlbaum Associates.

Marr, D. (1982). *Vision.* San Francisco: Freeman.

Martin, J. & Thompson, J. (1997). Between scientism and relativism: Phenomenology and the new realism in psychology. *Theory and Psychology, 7* (5), 629-652.

Martin, R. D., & May, R. M. (1981). Outward signs of breeding. *Nature, 293*, 7-9.

Masterman, M. (1970). The nature of a paradigm. In I. Lakatos and A. Musgrave (Eds.). *Crticism and the growth of knowledge.* Cambridge: Cambridge University Press.

Marx, M. H., & Cronan-Hillax, W. A. (1987). *Systems and theories in psychology.* New York: McGraw Hill.

Mathes, E. W. (1991). A cognitive theory of jealousy. In P. Salovey (Ed.). *The psychology of jealousy and envy.* New York: The Guilford Press.

Maynard Smith, J. (1974). The theory of games and the evolution of animal conflicts. *Journal of Theoretical Biology, 47*, 209-221.

Maynard-Smith, J., Burian, R., Kauffman, S., Alberch, P., Campbell, J., Goodwin, B., Lande, R., Raup, D., and Wolpert, L. (1985). Devolpmental constraints and evolution. *Quaterly Review of Biology, 60*, 265-87.

Maxwell, N., (1980). Science, reason, knowledge and wisdom. *Inquiry, 23,*

Maxwell, N. (1984). *From Knowledge to Wisdom: A revolution in the aims and methods of science.* NY: Basil Blackwell.

Mayr, E. (1982). *The growth of biological thought: Diversity, evolution, and inheritance.* Cambridge, Massachusetts: Belknap Press.

Mayr, E. (1983). How to carry out the adaptationist program? *The American Scientist, 121*, 324-334.

Meehl, P. (1967). Theory testing in psychology and physics: A methodological paradox. *Philosophy of Science, 34*, 103-115.

Meehl, P. (1978). Theoretical risks and tabular asterisks: Sir Karl, Sir Ronald, and the slow progress of soft psychology. *Journal of Consulting and Clinical Psychology, 46*, 806-834.

McCauley, R. N. (1986). Intertheoretic relations and the future of psychology. *Philosophy of Science, 53*, 179-199.

McClanahan, L. L., Ruibal, R., & Shoemaker V. H. (1994). Frogs and toads in deserts. *Scientific American, 270* (3), 64-72.

McDonald, K. (1992). Warmth as a developmental construct: An evolutionary analysis. *Child Development, 63*, 753-773.

McDougall, W. (1908). *An introduction to social psychology*. London: Metheun & Co limited.

McGinn, C. (1989). *Mental Content*. Oxford: Basil Blackwell.

McGlone, M. S. & Webb, C. J. (1981). Selective forces influencing the evolution of divaricating plants. *New Zealand Journal of Ecology, 4*, 20-28.

McGrew, W. C. (1992). *Chimpanzee material culture: implications for human evolution*. Cambridge: Cambridge University Press.

McMullin, E. (1984). A case for scientific realism. In J. Leplin (Ed.). *Scientific realism*. Berkeley: University of California Press. Pp 8-41

McMullin, E. (1993). Rationality and paradigm change in science. In P. Horwich (Ed.). *World changes: Thomas Kuhn and the nature of science*. Pp55-81 Cambridge, Massachusetts : MIT Press.

Megarry, T. (1995). *Society in prehistory: the origins of human culture*. New York: New York University Press.

Mellars, P. A. (1989). Major issues in the emergence of modern humans. *Current Anthropology, 30*, 349-385.

Midgeley, M. (1980). Gene juggling. In A. Montagu (Ed.). *Sociobiology examined*. New York: Oxford University Press.

Millikan, R. G. (1984). *Language, thought, and other biological categories*. Cambridge: MIT Press.

Millikan, R. G. (1986). Thought without laws; cognitive science with content. *Philosophical Review (1)*, 47-80.

Millikan, R. G. (1989a). Biosemantics. *Journal of Philosophy, 86*, (6), 281-297.

Millikan, R. G. (1989b). In defence of proper functions. *Philosophy of Science, 56*, 288-302.

Millikan, R. G. (1993). *White queen psychology and other essays for Alice*. Cambridge, Massachusetts: MIT Press.

Miller, G. A. (1992). The constitutive problem of psychology. In S. Koch & D. E. Leary (Eds.). *A century of psychology as a science*. Washington: American Psychological Association.

Mithin, S. (1996). *The prehistory of the mind: A search for the origins of art, religion, and science*. London: Thames & Hudson.

Mithin, S. (1997). The early pre-history of human social behavior: Issues of archaelogical inference and cognitive evolution. In W. G. Runciman, J. Maynard-Smith, & R. I. M. Dunbar (Eds.). *Evolution of social behavior: patterns in primates and man*.

Moller, A. P. (1987). Mate guarding in the swallow *Hirundo rustica*. *Behavioural Ecology and Sociobiology*, 21, 119-123.

Moller, A. P. (1988). Paternity and paternal care in the swallow, *Hirundo rustica*. *Animal Behaviour, 36*, 996-1005.

Montgomery, R. (1995). Explanation and evaluation in cognitive science. *Philosophy of Science, 62*, 261-282.

Mulkay, M. (1979). *Science and the sociology of knowledge*. London: George Allen and Unwin.

Newell, A. (1990). Unified theories of cognition. Cambridge, Mass: Harvard University Press.

Newton-Smith, W. H. (1981). *The rationality of science*. Boston: Routledge & Kegan Paul.

Nisbett, R. E., and Ross, L. (1980). *Human inference: strategies and shortcomings of social judgement*. Englewood Cliffs, NJ: Prentice-Hall.

O'Donohue, W. (1993). The spell of Kuhn on psychology: an exegetical elixir. *Philosophical Psychology, 6*, 267-287.

Orgel, L. E. & Crick, F. H. C. (1980). Selfish DNA: the ultimate parasite. *Nature, 284*, 604-607.

Orians, G. H. (1992). Evolved responses to landscapes. In J. H. Barkow, L. Cosmides, & J. Tooby (Eds.). *The adapted mind: evolutionary psychology and the generation of culture*. New York: Oxford University Press.

Ostrom, J. H. (1974). Archaeopteryx and the origin of bird flight. *Quarterly Review of Biology, 49*, 27-47.

Oyama, S. (1985). *The ontogeny of information*. Cambridge: Cambridge University Press.

Oyama, S. (1991). Bodies and minds: Dualism in evolutionary theory. *Journal of Social Issues, 47* (3), 27-42.

Packer, M. J. (1985). Hermeneutic inquiry in the study of human conduct. *American Psychologist*, October, 1081-1093.

Panksepp, J. (1981). Towards a general psychobiological theory of emotion. *Behavioural and Brain Sciences, 5*, 407-467.

Parker, G. A. (1982). Why are there so many tiny sperm? Sperm competition and the maintenance of two sexes. *Journal of Theoretical Biology, 96*, 281-294.

Parrott, W. G. (1991). The emotional experiences of envy and jealousy. In P. Salovey (Ed.). *The psychology of jealousy and envy*. (pp 3-28). New York: The Guilford Press.

Piatelli-Palmarini, M. (1989). Evolution, selection, and cognition: from 'learning'to parameter setting in biology and the study of language. *Cognition, 31*, 1-44 .

Pinker, S. (1994). *The language instinct*. London: Penguin Books.

Pinker, S. & Bloom, P. (1990). Natural language and natural selection. *Behavioural and Brain Sciences, 13*, 707-784.

Plutchik, R. (1980). *Emotion: a psychoevolutionary synthesis*. New York: Harper & Row.

Politzer, G., & Nguyen-Xuan, A. (1992). Reasoning about conditional promises and warnings: Darwinian algorithms, mental models, relevance judgements or pragmatic schemas. *Quarterly Journal of Experimental Psychology, 44* (3), 401-422.

Popper, K. (1959). *The logic of scientific discovery.* London: Hutchinson & Co

Popper, K. (1963). *Conjectures and refutations: the growth of scientific knowledge.* London: Routlege

Povinelli, D. J. (1993). Reconstructing the evolution of mind. *American Psychologist, 48,* 493-509.

Povinelli, D. J., Nelson, K. E., & Boysen, S. J. (1990). Inferences about guessing and knowing by chimpanzees. *Journal of comparative psychology, 104,* (3), 203-210.

Povinelli, D. J. (1995.). Theory of mind: Evolutionary history of a cognitive specialisation. *Trends in Neurosciences, 18* (9), 418-424.

Previde, E., Colombetti, M., Poli, M., Spada, E. C. (1992). The minds of organisms: some issues about animal cognition. *International Journal of Comparative Psychology, 6* (2), 79-100.

Putnum, H. (1982). Why reason can't be naturalised. *Synthese, 52,* 3-23.

Quine, W. V. O. (1969). *Ontological relativity and other essays.* New York: Columbia University Press.

Rachels, J. (1990). *Created from animal: the moral implications of Darwinism.* Oxford: Oxford University Press.

Railton, P. (1981). Probability, explanation, information. *Synthese, 48,* 233-256.

Railton, P. (1986). Moral realism. *The Philosophical Review, 95* (2), 163-207.

Raup, D. M. (1991). *Extinction: Bad genes or bad luck?* New York: W. W. Norton.

Reed, E. (1978). *Sexism and science.* New York: Pathfinder Books.

Read, S. J. and Marcus-Newhall, A. (1993). Explanatory coherence in social explanations: a parallel distributed processing account. *Journal of Personality and Social Psychology, 65* (3), 429-447.

Richards, R. J. (1987). *Darwin and the emergence of evolutionary theories of* mind and *behaviour.* Chicago: University of Chicago Press.

Richards, R. J. (1974). The innate and the learned: The evolution of Konrad Lorenz's theory of instinct. *Philosophy of the Social Sciences, 4,* 111-133.

Richerson, P. J., & Boyd, R. (1987). The role of evolved predispositions in cultural evolution: or, human sociobiology meets Pascal's wager. *Ethology & Sociobiology, 10,* 195-219.

Ristau, C. (Ed.). (1991). *Cognitive Ethology: The minds of other animals.* Erlbaum.

Ristau, C. (1991). Before mind reading: Attention, purposes and deception in birds? In A. Whiten (Ed.). *Natural theories of mind: Evolution, development and simulation of everyday mindreading.* Oxford: Basil Blackwell.

Robinson, D. (1985). *Philosophy of psychology.* New York: Columbia University Press.

Romanes, G. (1881). *Animal intelligence.* London: Kegan Paul.

Romanes, G. (1888). *Mental evolution in the mind: origins of human faculty.* London: Kegan Paul.

Rose, S., Kamin, L. J. and Lewontin, R. C. (1984). *Not in our genes: biology, ideology and human nature.* New York: Penguin Books.

Royce, J. R. (1987). A strategy for developing unifying theory in psychology. In, A. W. Staats, & L. P. Mos (Eds.). *Annals of theoretical psychology, 5,* New York: Plenum Press.

Ruse, M. (1981). *Is science sexist?* Dordrecht: D. Reidal Publishing Company.

Ruse, M. (1984). Charles Darwin and group selection. In R. N. Brandon & R. M. Burian (Eds.). *Genes, organisms, populations: controversies over the units of selection.* Cambridge, Massachusetts: MIT Press.

Ruse, M. (1987). Is sociobiology a new paradigm? *Philosophy of Science, 54,* 98-104.

Sahlins, M. (1976). *The use and abuse of biology.* Ann Arbor: University of Michigan Press.

Salovey, P., & Rothman, A. (1991). Envy and jealousy: self and society. In P. Salovey (Ed.). *The psychology of jealousy and envy.* (pp 271-287.). New York: The Guilford Press.

Salmon, W. (1984). *Scientific explanation and the causal structure of the world.* Princeton, NJ: Princeton University Press.

Salmon, W. (1985). Conflicting concepts of scientific explanation. *Journal of Philosophy, 82,* 651-655.

Salmon, W. (1989). Four decades of scientific explanation. In P. Kitcher & W. Salmon (Eds.). *Scientific explanation.* Minneapolis: Minneapolis Press.

Secord, P. F. (1990). The need for a radically new human science. In D. N. Robinson & L. P. Mos (Eds.). *Annals of theoretical psychology, 6,* New York: Plenum Press.

Seligman, M. E. P. & J. L. Hager (Eds.). (1972). *Biological boundaries of learning.* New York: Century - Appleton - Crofts.

Shallice, T. (1988). *From neuropsychology to mental structure.* Cambridge: Cambridge University Press.

Sharpsteen, D. J. (1991). The organization of jealousy knowledge: romantic jealousy as a blended emotion. In P. Salovey (Ed.). *The psychology of jealousy and envy.* (pp 31-52). New York: The Guilford Press.

Shaver, P., Schwartz, J., Kirson, D., & O'Conner, C. (1987). Emotion Knowledge: Further exploration of a prototype approach. *Journal of Personality and Social Psychology, 52,* 1061-1086.

Shepard, R. N. (1984). Ecological constraints on internal representation: Resonant kinematics of perceiving, imagining, thinking, and dreaming. *Psychological Review, 91,* 417-447.

Shepard, R. N. (1992). The perceptual organization of colors: An adaptation to regularites of the terrestrial world? In J. Barkow, L. Cosmides, & J. Tooby (Eds.). *The adapted mind* (pp 495-532) New York: Oxford University Press.

Silverman, I., & Eals, M. (1992). Sex differences in spatial abilities: Evolutionary theory and data. In L. Cosmides, J. Tooby, & J. Barkow (Eds.). *The adapted mind.*

Simpson, J. A., & Kenrick, D. T. (Eds.). (1997). *Evolutionary social psychology.* New Jersey: Lawrence Erlbaum Press.

Singer, P. (1995). *Animal Liberation* (second edition). London: Pimlico.

Skinner, B. F. (1938). *The behaviour of organisms.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1974). *About behaviorism*

Smith, R. L. (Ed). (1984). *Sperm competition and the evolution of animal mating systems.* London: Academic Press.

Smuts, R. W. (1991). The present also explains the past. *Ethology and Sociobiology, 12,* 77-82.

Sociobiology study group of science for the people (1978). Another biological determinism. In A. L. Caplan (Ed.). *The sociobiology debate* (pp280-291). New York: Harper & Row.

Sober, E. (1981). The evolution of rationality. *Synthese, 46,* 95-120.

Sober, E. (1984). *The nature of selection: evolutionary theory in philosophical focus.* Cambridge, Massachusetts: MIT Press.

Sober, E. (1985). Panglossian functionalism and the philosophy of mind. *Synthese, 64,* 165-193.

Sober, E. (1993a). *Philosophy of biology.* Boulder: West view Press.

Sober, E. (1993b). Evolutionary altruism, psychological egoism, and morality: disentangling the phenotypes. In M. H. Nitecki & D. V. Nitecki (Eds.). *Evolutionary Ethics.* New York

Spelke, E. S. (1991). Physical knowledge in infancy: reflection on Piaget's theory. In S. Carey and R. Gelman (Eds.). *The epigenesis of mind: Essays on biology and cognition.* New Jersey: Lawrence Erlbaum Associates.

Sperber, D. (1994). The modularity of thought and the epidemiology of representations. In L. A. Hirschfeld & S. A. Gelman (Eds.). *Mapping the mind: domain specificity in cognition and culture.* Cambridge: Cambridge University Press.

Sperber, D. (1996). *Explaining culture: A naturalistic approach.* Oxford: Blackwell.

Staats, A. W. (1989). Unificationism: philosophy for the modern disunified science of psychology. *Philosophical Psychology, 2,* 143-164.

Sterelny, K. (1990). *The representational theory of mind.* Cambridge: Blackwell.

Sterelny, K. (1992). Evolutionary explanations of human behaviour. *Australian Journal of Philosophy, 70* (2), 156-173.

Stevens, A., & Price, J. (1996). *Evolutionary psychiatry: A new beginning.* New York: Routledge.

Stich, S. P. (1985). Could man be an irrational animal? Some notes on the epistemology of rationality. In H. Kornblith (Ed.). *Naturalizing epistemology.* Cambridge, MA: MIT Press.

Stove, D. (1995). *Darwinian fairytales.* Aldershot: Avebury.

Strongman, K. T. (1996). *The psychology of emotion: theories of emotion in perspective.* Chichester: John Wiley and sons Inc.
Symons, D. (1989). A critique of Darwinian anthropology. *Ethology & Sociobiology, 10,* 131-144.
Symons, D. (1990). Adaptiveness and adaptation. *Ethology and Sociobiology, 11,* 427-444.
Symons, D. (1992). On the use and misuse of Darwinism in ht estuey of human behaviour. In J. Barkow, L. Cosmides, & J. Tooby (Eds.). *The adapted mind* (137-159). New York: Oxford University Press.
Thagard, P. (1978). The best explanation for theory choice. *Journal of Philosophy, 75,* 76-92.
Thagard, P. (1989). Explanatory coherence. *Behavioural and Brain Sciences, 12,* 435-502.
Thagard, P. (1988). *Computational philosophy of science.* Cambridge: MIT Press
Thagard, P. (1992). *Conceptual revolutions.* Princeton, NJ: Princeton University Press.
Thornhill, R. & Alcock, J. (1983). *The evolution of insect mating systems.* Cambridge: Harvard University Press.
Tinbergen, N. (1963). On the aims and methods of ethology. *Zeitschrift fur Tierpsychologie, 20,* 410-433.
Tinbergen, N. (1973). *The animal in its world: Explorations of an ethologist 1932-1972 vol 1 & 2.* London: George Allen & Unwin Ltd.

Tomasello, M. (1996). The question of chimpanzee culture. In R. W. Wrangham, W. C. McGrew., F. B. M. de Waal, & P. T. Heltne (Eds.). *Chimpanzee Cultures.* Cambridge: Harvard University Press.
Tomasello, M., Davis-Dasilva, M., Camak, L., & Bard, K. (1987). Observational learning of tool use by young chimpanzees. *Human Evolution, 2,* 175-183.
Tomasello, M., Kruger, A. C., & Ratner, H. H. (1993). Cultural learning. *Behavioral and Brain Sciences, 16,* 495-552.
Tooby, J. & Cosmides, L. (1989). Evolutionary psychology and the generation of culture, part 1. *Ethology and Sociobiology, 10,* 29-49.
Tooby, J. & Cosmides, L. (1990a). On the universality of human nature and the uniqueness of the individual: the role of genetics and adaptation. *Journal of Personality, 58,* 17-67.
Tooby, J. & Cosmides, L. (1990b). The past explains the present: emotional adaptations and the structure of ancestral environments. Ethology & Sociobiology, 11, 375-424.
Tooby, J. & Cosmides, L. (1992). Psychological foundations of culture. In J. Barkow, L. Cosmides, & J. Tooby, (Eds.). *The adapted mind* pp19-136. New York: Oxford University Press.
Tooby, J., & Cosmides, L. (1996). Friendship and the banker's paradox: other pathways to the evolution of adaptations for altruism. In W. G. Runciman,

J. Maynard-Smith & R. I. M. Dunbar (Eds.). *Evolution of social behavior in primates and man*. Oxford: Oxford University Press.

Toulmin S. & Leary, D. E. (1992). The cult of empiricism in psychology, and beyond. In S. Koch & D. E. Leary (Eds.). *A century of psychology as science.* Washington, DC: American psychological Association.

Tavris, C. (1992). *The mismeasure of woman* New York: Simon & Schuster.

Travis, C. B. & Yeager, C. P. (1991). Sexual selection, parental investment, and sexism. *Journal of Social Issues, 47* (3), 117-129.

Trivers, R. L. (1971). The evolution of reciprocal altruism. *Quarterly Review of Biology, 46*, 35-57.

Trivers, R. L. (1972). Parental investment and sexual selection. In B. Campbell (Ed.) *Sexual selection and the descent of man*. Chicago: Aldine-Atherton.

Trivers, R. L. (1974). Parent-offspring conflict. *American Zoologist, 14*, 249-264.

Trout, J. D. (1991). Introductory essay: reductionism and the unity of science. In R. Boyd, P. Gasper, & J. D. Trout (Eds.). *The philosophy of science.* Cambridge, MA: MIT Press.

Turke, P. W. (1990). Which humans behave adaptively and why does it matter? *Ethology and Sociobiology, 11*, 305-339.

Van den Berghe, P. (1983). Human inbreeding avoidance: culture in nature. *Behavioral and Brains Sciences, 6*, 91-124.

Van Fraassen, B. C. (1977). The pragmatics of explanation. *American Philosophical Quarterly, 14*, 143-150.

Van Fraassen, B. C. (1980). *The scientific image*. Oxford: Oxford University Press.

Van Fraassen, B. C. (1984). To save the phenomena. In J. Leplin (Ed.). *Scientific Realism*. Berkeley: University of California Press. 250-261

Vining, D. R. (1986). Social versus reproductive success: The central theoretical problem of human sociobiology. *Behavioral and Brain Sciences, 9*, 167-216.

de Waal, F. M. M. (1982). *Chimpanzee politics*. London: Jonathan Cape.

de Waal, F. M. M. (1991). The chimpanzee's sense of social regularity and its relation to the humans sense of justice. *American Behavioural Scientist, 34* (3), 335-349.

de Waal, F. M. M. (1996). *Good natured: the origins of right and wrong in humans and other animals*. Cambridge Massachusetts: Harvard University Press.

de Waal, F. M. M. & Luttrell, L. M. (1987). Mechanisms of social reciprocity in three primate species: Symmetrical relationship characteristics or cognition? *Ethology & Sociobiology, 9*, 101-118.

Wachtel, P. L. (1980). Investigation and its discontents: Some constraints on progress in psychological research. *American Psychologist, 35* (5), 399-408.

Walker, S. (1983). *Animal thought*. London: Routledge & Kegan Paul.

Wallace, A. F. (1891). *Natural selection and tropical nature.* London: Macmillan and Co.

Watson, J. D. (1967). *The double helix*. New York: Norton.

White, M. & Gribbin, J. (1996). *Darwin: A life in science*. London: Simon & Schuster.

White, G. L. (1981). Some correlates of romantic jealousy. *Journal of Personality, 49*, 129-147.

White, G. L., & Mullen, P. E. (1989). *Jealousy: Theory, research, and clinical strategies.* New York: The Guilford Press.

Whiten, A. & Byrne, R. W. (1988). Tactical deception in primates. *Behavioral and Brain Sciences, 11*, 233-273.

Williams, G. C. (1966). *Adaptation and natural selection*. Princeton: Princeton University Press.

Williams, G. C. (1992). *Natural selection: Domains, levels, and challenges*. New York: Oxford University Press.

Wilson, D. S. & Sober, E. (1994). Reintroducing group selection to the human behavioural sciences. *Behavioural and Brain Sciences, 17*, 585-654.

Wilson, D. S. (1997). Incorporating group selction into the adaptationist program: A case study involving human descion making. In J. A. Simpson & D. T. Kenrick (Eds.). *Evolutionary social psychology*.(pp 345-387). New Jersey: Lawrence Erlbaum Associates.

Wilson, E. O. (1975). *Sociobiology: The new synthesis*. Cambridge, Massachusetts: Harvard University Press.

Wilson, E. O. (1978). *On human nature*. Cambridge, Massachusetts: Harvard University Press.

Wilson, E. O. (1994). *Naturalist*. Washington, DC: Island Press.

Wilson, M., & Daly, M. (1992). The man who mistook his wife for a chattel. In J. Barkow, L. Cosmides, & J. Tooby (Eds.). *The adapted mind: evolutionary psychology and the generation of culture.* (pp289-322). New York: Oxford University Press.

Woolgar, S. (1988). *Science: the very idea*. London: Tavistock Publications.

Wright, L. (1973). Functions. *Philosophical Review, 82*, 139-168.

Zammer, V. L., & Fischer, A. H. (1995). The social regulation of emotions in jealousy situations. *Journal of Cross-Cultural Psychology, 26* (2), 189-208